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TRANSACTIONS

OF THE

INSTITUTION

OF

CIVIL ENGINEERS.

VOLUME III.

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CONTENTS.

		PART	PAGE
I.	*On Steam Boilers and Steam Engines. Part* I. *By* JOSIAH PARKES, *M. Inst. C. E.*	I.	1
II.	*Ditto. Part* II. *By Ditto*	II.	49
III.	*On the Preparation, Properties, and Uses of Turf and Turf Coke. By* CHARLES WYE WILLIAMS, *Assoc. Inst. C. E.*	„	163
IV.	*An Investigation into the Power of Locomotive Engines, and the Effect produced by that Power at different Velocities. By* PETER BARLOW, *F.R.S., Hon. M. Inst. C. E., &c. &c.*	III.	183
V.	*Description of a Sawing Machine for Cutting Railway Bars. By* JOSEPH GLYNN, *F.R.S., M. Inst. C. E.*	„	197
VI.	*On the Expansion of Arches. By* GEORGE RENNIE, *F.R.S., M. Inst. C.E., &c. &c.*	„	201
VII.	*Description of the State of the Suspension Bridge at Montrose, after it had been rendered impassable by the Hurricane of the* 11*th October,* 1838; *with remarks on the construction of that and other Suspension Bridges, in reference to the action of violent Gales of Wind. By* C. W. PASLEY, *C. B., Major-General Royal Engineers, F.R.S., Hon. M. Inst. C. E., &c.*	„	219
VIII.	*On the Supply of Water from Wells in the London Basin, with an Account of the Sinking of the Well at the Reservoir of the New River Company, in the Hampstead Road. By* ROBERT W. MYLNE	„	229

CONTENTS.

		PART	PAGE
IX.	*Account of the Gravesend Pier. By* WILLIAM TIERNEY CLARK, *F.R.S., M. Inst. C.E.*	III.	245
X.	*On the Action of Steam in Cornish Single Pumping Engines. By* JOSIAH PARKES, *M. Inst. C.E.*	IV.	257
XI.	*On Setting-out Railway Curves. By* CHARLES BOURNS, *Assoc. Inst. C.E.*	„	295
XII.	*On the Locomotive Engines of the London and Birmingham Railway. By* EDWARD BURY, *M. Inst. C.E.*	„	305
XIII.	*Account of the Mode of Construction adopted in Building a New Stone Bridge over the River Lea, at Stratford-le-Bow. By* JOHN BALDRY REDMAN, *Grad. Inst. C.E.* . .	V.	343
XIV.	*Observations on the Effects produced by Wind on the Suspension Bridge over the Menai Strait, more especially as relates to the Injuries sustained by the Roadways during the Storm of January* 7, 1839, *together with brief Notices of various Suggestions for Repairing the Structure. By* W. A. PROVIS, *M. Inst. C.E.*	„	357
XV.	*Account of the Alterations made in the Structure of the Menai Bridge, during the Repairs, in consequence of the damage it received from the Gale of January* 7, 1839. *By* THOMAS JAMES MAUDE, *Grad. Inst. C.E.*	„	371
XVI.	*Description of a Cofferdam adapted to a hard bottom, used in Excavating Rock from the Navigable Channel of the River Ribble; designed for the Ribble Navigation. By* DAVID STEVENSON, *C.E., Edinburgh*	„	377
XVII.	*Memoir on the Practicability of Shortening the Duration of Voyages, by the adaptation of Auxiliary Steam Power to Sailing Vessels. By* SAMUEL SEAWARD, *F.R.S., M. Inst. C.E.*	„	385

CONTENTS.

		PART	PAGE
XVIII.	*On the Percussive or Instantaneous Action of Steam and other Aeriform Fluids. By* JOSIAH PARKES, *M. Inst. C. E.*	V.	409
XIX.	*On the circumstances under which Explosions frequently occur in Steam Boilers, and the causes to which such Explosions may be assigned. By* CHARLES SCHAFHAEUTL, *M. D., Assoc. Inst. C. E.*	„	435
XX.	*Remarks on the Duty of the Steam Engines employed in the Mines of Cornwall at different periods. By* JOHN S. ENYS, *Assoc. Inst. C. E*	„	449
Index to Volumes I. II. and III.			467
Officers, Members, &c.			481—497

TRANSACTIONS.

I.—*On Steam-Boilers and Steam-Engines.*

By JOSIAH PARKES, M.Inst.C.E.

In a former paper* "On the Evaporation of Water from Steam-boilers," I laid before the Institution several tables shewing the evaporative product of three distinct classes of boiler in common practice; with remarks elucidating various phenomena which attend combustion, and the treatment of fuel. I now propose to investigate and compare the peculiar properties of those and other boilers, as exemplified in their practice; to show their points of agreement and disagreement; to exhibit their respective merits and demerits as evaporative vessels; to point out some general laws which may contribute to give greater uniformity to the results of evaporation from any assigned heated surface of boiler, and enable the employer of a boiler, not only to ascertain if he is using his fuel economically or wastefully, but to apply a remedy should he find his practice imperfect.

I stated, in the paper referred to, that "had we a full knowledge of the relative strength of the coals employed, and full information of the respective areas of the grates, and of the heated surface of the boilers, an investigation into the *causes* of the superiority of one boiler over another, both as regards their *evaporative power* and *evaporative economy*, might be instructively, if not successfully entered upon." The tables in that paper give the amount of evaporation, the weight of fuel consumed, and the temperature of the water entering a

* Transactions of the Inst. C. E. Vol. II.

variety of boilers; by further tabulating the dimensions of the several boilers, the area of their grates, the area of their heat-absorbing surfaces, with the rates of combustion and evaporation, we have every element for the proposed investigation and comparison, excepting one, viz. the relative strength of the coals employed. This is a deficiency which cannot be supplied, but the absence of that knowledge—interesting and useful as it would be—will not be found to affect those general principles which conduce to the economy, durability, and safety of steam-boilers, principles which depend on the construction and proportion of the vessels to the work required of them, combined with their management and the application of the heat.

Evaporation is the measure of the *useful effect* obtained from any given weight of fuel of any kind. By comparing, therefore, the product from one boiler with that from another, and by observing the results of different modes of practice on the same boiler, the value of any arrangement of the parts of an evaporative vessel, with its fitness for its intended purpose, can be ascertained. For the sake of clearness, I shall confine my remarks on the steam-boilers under review to Part I. of this paper, so far as regards the elucidation of their qualities, and performance as evaporative vessels, their general management, and the phenomena attending the combustion of the fuel.

Evaporation, when combined with the knowledge of the duty done by an engine, is also the measure of the *useful effect* of a given weight of water in the shape of steam, as an agent of mechanical power; and it furnishes the only true test of the value of different systems of applying steam. Were we ignorant of all the other properties of steam besides those of its elasticity, its expansiveness, and its condensibility, we could still—if acquainted with the weight of water which, in the shape of steam, has passed from the boiler through the cylinder of an engine, and produced a certain effect—arrive at a practical and precise determination of the efficiency of that steam as a motive force, and of the relative excellences of different engines.

During the infancy of the steam-engine the quantity of water, as steam, necessary to produce a given effect or *duty*, was as much the object of consideration by the chief improvers of the engine, as the quantity of coal consumed. It formed the subject of continual trial and laborious study, both experimentally and practically, by Smeaton, and it was the basis of Watt's discoveries. Smeaton, with his characteristic accuracy of detail, has recorded the particulars of the boilers, evaporation, consumption of fuel, and duty of the Long Benton

engine, which was the model, and may be called the gauge of the performance of the best engines on the atmospheric principle. I have transferred the particulars of these boilers and engine to the ensuing tables, not only as interesting matters of history, but as useful evaporative and comparative facts.

I am acquainted with no other recorded trial of the whole quantity of steam passing through Watt's double engine than that made by himself and Rennie, at the Albion Mills; I have also extracted these particulars, and tabulated them as affording a specimen of that which was considered by Watt himself as the full performance of his rotative engine. The duty done by the engine at the Albion Mills, as estimated by the quantity of *steam* used, has probably not been exceeded by any one of the same unexpansive kind since constructed by Watt, or other makers, though in a few instances less *coal* may have been consumed through more skilful management, and the use of better boilers.

The bushel of coal was originally fixed by Watt as the measure of the duty done by the pumping engine. It was presumed that the engine which consumed the least coal, required the least steam, and doubtless, on the aggregate of engines of the same class having similar proportions of parts and boiler, and using the same kind of coal, this would be true. For commercial purposes, also, when the maker was paid, as Watt originally was, a certain share of the saving in coals which he effected over other engines, the weight or measure of fuel burnt, compared with its effect, formed the most convenient criterion of the merit of the engine between its maker and its employer. Hence originated the denomination of duty, as the weight of water raised one foot by a bushel of coal. The constant association of this standard in the minds of engineers, has probably been the cause that, notwithstanding the progressive improvements in the performance of pumping engines, in Cornwall especially, we have remained in utter ignorance of the progressive diminution in their consumption of *steam*. From neglecting to determine this indispensable datum, it has chiefly arisen that the reports of the Cornish engineers, as to the duty done by the modern high pressure expansive engines, have been so long either discredited or doubted. A loophole has been left for disbelief, by their referring, from fixed habit, to an *uncertain*, rather than to a *certain* standard of comparison, and though in a district of England where every engineer, and every employer of an engine, (persons the most interested in ascertaining, and the most capable of determining, and profiting by the truth,) have been for years satisfied as to the preeminent superiority of the high pressure expansive over Watt's low pressure engine, never-

theless, strong assertion has never been idle beyond the confines of that district, in attributing the reported difference in the performance of these two engines, to a false or incorrect mode of taking the duty, to some miraculous strength of the coals employed, or to some mysterious and unexplained fertility of steam in the Cornish boiler. On the other hand, the advocates of the high pressure expansive system may, from the same want of positive data, have somewhat exaggerated the dynamic advantage derivable from that method of employing steam.

Mr. Wicksteed*, by his admirable *experimentum crucis*, viz., the weighing of the water actually raised by a bushel of coals, has demonstrated the near coincidence of the Cornish system of ascertaining the duty with the real performance of the engine. Still, the knowledge of the steam consumed in doing the duty, remained to be unfolded. This I have at length the means of developing from experiments on two Cornish engines, which will establish that boiler and engine in their true situation in the scale of economy. These, with similar facts, indicated by a knowledge of the evaporation and duty of several other classes of engines, I propose to consider separately in a second part of this paper.

PART I.

ON THE QUALITIES OF STEAM-BOILERS, AND ON THE INFLUENCE EXERCISED OVER EVAPORATION BY THEIR PROPORTIONS AND PRACTICAL MANAGEMENT.

My own experiments on these subjects commenced more than twenty years since, and I fortunately preserved, in each instance, the particulars of the dimensions of the boilers and grates operated upon. I very early perceived that the *completeness*, and the *rate* of combustion, were important elements in evaporative economy, and also that the *proportion of the grates* to the combustion effected upon them, as well as to the whole heat-absorbing surface, exercised a considerable influence over the results; nor could I fail to observe that the greatest results were obtained when these elements bore some certain ratio to each other. Still I did not consider that the amount of my experience, or the number of my experiments, operating as I did, on one form of boiler alone, (the *wagon*,) justified the laying down a rule which could be common to all the possible forms of boiler It appeared to me that the boiling apparatus of a steam-engine was

* Trans. Inst. C. E. Vol. II.

capable of being thrown into an infinite variety of shapes; that the combustion of the fuel might be employed at an intensity far exceeding that in the then common practice; and I thought it very possible that some strange shape and condition of boiler might be invented, which would set a limit to practice in rapid combustion, whilst I considered my own practice at Warwick to have reached very nearly the extreme of slow combustion. I also thought that varieties of boilers, or systems of practice might be introduced, which would serve as intermediate specimens, and connect the two extremes. Time has justified these expectations, and by this delay I have avoided falling into the too common error of generalizing from insufficient data, or isolated facts,—an error productive of much mischief to the progress of science, as the reputation of an author, coupled with the indisposition of the mass of men to think for themselves, frequently occasion very imperfect, and even erroneous doctrines to be received as law. It is not till facts, unexceptionable in their nature, and multiplied in number, stand forth in all the clearness of truth, that the theory of their action can be investigated with certainty and success.

To my own experiments * detailed in my former paper on wagon boilers, I have now added Smeaton's at the Long Benton, and Watt's at the Albion Mills engines.

Since the period referred to, the *locomotive* engine has sprung into being, and its boiler has taken a place so important as an evaporative vessel, that to an enquirer on this subject it is full of interest. It touches the margin of the highest possible intensity of combustion in its use of fuel; it exceeds all other boilers in evaporative power, and presents in its various properties, contrasts so striking with every other class of boiler, that the analysis of its properties and effects is eminently instructive. M. de Pambour having recorded the full particulars of the boilers and grates belonging to the engines on which he experimented, I am able to examine them in reference to their qualities and evaporation, of which I gave a table in my former paper.

To this class I have now added Mr. Wood's experiments on the boiler of an engine detailed in the third edition of his Work on Railways, page 333.

The evaporative qualities of the *Cornish* boiler have been but little enquired into. This particular form of boiler, long established as it has been in a district

* The term *experiment* is used throughout this paper in the sense of an *observation of facts* attending the common every day working of the boilers, except in a few cases which are specially mentioned.

of England to which high pressure steam used expansively has been pretty much confined, and which has there supplanted all other forms as the best adapted for its purpose, is the more interesting, and the more worthy of investigation, from its forming part of a system of generating and employing steam, the economy of which has been silently, slowly, but surely progressive. A fair test of its effects as compared with other boilers will be found supplied in Table IV. of my former paper, and having since obtained the measurement of the boilers and grates appertaining to the experiments therein recorded, I am now enabled to furnish and to analyse all the principles on which those effects depend.

In addition to these I have tabulated the particulars of two other boilers whose surfaces and consumption of fuel are stated in Mr. Henwood's paper of last session, as they serve, even without a knowledge of their evaporation, (which unfortunately does not accompany them,) to determine the proportions of boilers, and other useful data connected with the practice of the Cornish engineers.

The *marine* boiler is another distinctive class as regards structure, and arrangement of parts. These it has gradually assumed by the apparently common consent, and from the united experience of marine engine makers; it may therefore be supposed to combine the peculiar characteristics which fit it in their judgment for the locality and circumstances of a steam-vessel,—these require that the boiler should possess, in a preeminent degree, the three prime qualities of safety, durability, and economy, together with an abundant evaporative power. I greatly regret that I have no data by which to compare its properties and practical results in the generation of steam with other boilers, as I cannot meet with any authenticated experiments on its evaporative qualities in actual work on sea or rivers. Such experiments would be invaluable to the marine engineer, as demonstrative of the economy of his boiler in the use of fuel, a quality of first rate and growing consequence to steam navigation, and I need scarcely repeat that an acquaintance with the evaporation is the only sure test of the respective value of different kinds of fuel. The marine boiler is also beginning to be used in lieu of the common wagon boiler, and as there seems no reason why it should be unsuitable to a fixed engine, or other steam work employing steam at a very moderate pressure, provided the evaporative economy and durability of the two boilers are equal, it is to be regretted that its powers have not been tested, or if tested, not published *.

* It would require much care and experimental tact to make sufficiently exact trials of the evaporation produced from marine boilers on board *steam-vessels*. Evaporative experiments of short dura-

A variety of this class was brought under the notice of the Institution last session by Mr. Manby, in a paper on Collier's Patent Boiler. There is also recorded in Mr. Wood's Work on Railways, first edition, an experiment on a marine boiler with captain Ericson's fanning apparatus. It forms, however, no part of my plan to investigate the properties of these inventions; nor is it my province or desire to pronounce on the value of them, nor on that of numerous other ingenious contrivances for economizing fuel, which are to be found in partial use, or accounts of which are scattered in various publications. My motives in undertaking the present task have been to collect and make known authentic facts on the working of boilers and engines of established credit and notoriety, and so to exhibit them as to enable the employer of any boiler or engine, whether similar or dissimilar, to compare his practice with specimens of acknowledged and well attested merit.

The following Table is formed from the data referred to in my paper of last session with the additions above mentioned. By elimination, the relation or ratio which any one property or effect of the boilers bears to another is shewn; and the arrangement of the Table is such that each succeeding column or set of columns may be said to form links in the chain of evidence which terminate in the disclosure of the objects sought, viz. the principal conditions upon which evaporative economy is dependent.—Vide Table at the end of this paper.

Table I. From this Table, which presents a collective and digested view of all the experiments; of the relation which the several parts of the boilers bear to one another, and to the effects produced; of the amount and activity of the combustion, which may be termed the *calorific forces* to which boilers are exposed; as also of the modifications which these forces undergo through the structure and disposition of the several parts,—I shall select and place at the head of my observations on each class of boiler, those quantities and qualities which point themselves out as exerting the chief influence over the results. These sums are exponential of the practice, and of its effects, and they truly represent the relative calorific forces to which the several boilers are subject, and

tion on any boilers are of little practical value, and they require to be closely superintended by men accustomed to accurate observation. The necessity of frequently *blowing out*, or otherwise cleansing marine boilers, whether at work in sea or in rivers, would render such experiments very inconclusive, and most of these boilers are more liable to the *disease* of *priming* than any other class. Experiments, however, quite free from error, and of much value to the marine engineer, may be conducted on such boilers when supplying a fixed engine.

upon the degree of whose intensity and duration, the economy, durability, and safety of evaporative vessels must mainly depend.

I have reserved for separate examination, at the end of this paper, the question of the influence of *time* in producing the relative degrees of economy, as it requires special and careful analysis before a satisfactory determination of the value of that element can be arrived at. By *time*, I mean the relative periods of the duration of a given amount of heat about the boilers, and about equal areas of their surface. The facts arising out of this investigation will be found classified in Tables 3 and 5, whence they are transferred to Table 1.

Uniformity in the gradation of results from any one cause cannot be expected in a series of experiments on boilers of such various structure and strength of materials, but it will be apparent, from a single glance over the columns, that the rate of combustion, and its rate on each square foot of grate, very sensibly affect them; and one conclusion is inevitable, viz., that the practice of slow combustion is eminently conducive to economy in the treatment of fuel; nor can there be a doubt that the metal of which a boiler is composed is principally affected, both in its strength and durability, by the heat applied to it, so that these properties seem to be best preserved by, and to be the natural consequences of, slow combustion. Thus we shall find that the boilers, tested as to their merit by their respective evaporative economy, arrange themselves for consideration in the inverse order of the rate of combustion, and that the Cornish boiler, in this respect, obtains the highest rank.

A second though somewhat less regular coincidence between the operating causes and economical results, is indicated by the extent of surface exposed to absorb the heat supplied to the boiler. This element is subject to more or less of modification from a variety of contending circumstances, which will severally be noticed and considered in the ensuing observations on the practice of each class of boiler; but the table shews distinctly that economy of heat is promoted in some proportion of the extent of the absorbing surface, and the Cornish boiler again exhibits its superiority over all the others by the degree in which it possesses this element.

Time—used in the sense before explained, and which expresses the rate of combustion involved with the extent of the absorbing surface—is also greatly in favour of the Cornish boiler.

As that boiler and practice, therefore, afford the highest evaporative product, and appear to combine the greatest number of good qualities, and as it is re-

quisite that a standard should be adopted, in order to mark the scale of descent from the highest point of excellence yet attained in evaporative economy, I shall bring the other boilers to the test of the Cornish, with reference to their respective properties, and consider the Cornish results as unity.

CORNISH BOILER.

EXPONENTS *.

Column of Table.	lbs.	
48....	1	of coal burnt under one boiler in 44.08 seconds.
20....	3.46	of coal burnt on each square foot of grate per hour.
41....	1	of water evaporated by 1 square foot of heated surface per hour from 212°.
37....	11.82	of water evaporated by 1 lb. of coal from 212°.

The form, construction, usual dimensions, and mode of setting this boiler are too well known to require particular mention. It appears to possess all the principles necessary to resist internal and external pressure, due regard being paid to sufficient strength of materials. The fire is contained in an internal cylindrical tube, the pressure upon which being external, the greatest strength of resistance is obtained where it is most needed; nor can there be so great a lodgment of deposit upon the parts subjected to the most direct and intense heat as in the wagon boiler. The practice as regards the slow rate of combustion is eminently judicious, since it involves the necessity of employing a very extensive surface, or proportion of boiler to evaporation, for the absorption of the heat; which again, together with the requisite strength, occasions the use of several boilers of small diameters, rather than few of larger dimensions. By these means *time* is given for the complete combination of air with the heated fuel; *time* is given for the transmission of the heat through the metal; and *time* is given for the escape of the steam through the water. These are points of practice of great import to all boilers and to the well working of every species of steam-engine; but they are of still greater consequence to an engine using steam at a very elevated pressure, and employing that steam expansively. The durability

* The quantities brought from Table I. and thus placed at the head of the observations on each group or class of boilers, are those which form the principal points of contrast and comparison. They are indicative or exponential of the quality of the boiler and of the effects of the practice upon it; I have, therefore, termed them *exponents*.

of a boiler is greatly prolonged by using heat of a moderate intensity. Much thicker plates are indispensable in proportion as the pressures become elevated, and in proportion also to thickness of material, the conduction of heat is slower; hence the wisdom of reducing the combustion of the fuel to the slowest possible rate under all, but more particularly under high pressure boilers; and hence durability and safety are promoted in proportion to the diminution of calorific intensity; yet it would not be difficult to shew that, except by the Cornish engineers, the practice obtains too often of increasing both the extent and intensity of the fires almost in the ratio of the increased pressure of steam, from which practice arises one fertile source of short-lived boilers, and explosions.

It appears from the Table, column 45, that the Cornish engineers allow about 7 times as much surface as the general wagon boiler practice for the vaporization of equal weights of water in equal times, which is accompanied by a gain of between 30 and 40 per cent. on the mean of the 8 wagon boiler experiments, some of which, however, far exceed the average product of those boilers in common practice.

It must be carefully borne in mind that two very remarkable differences exist between the high and low pressure boilers under comparison; viz. the thickness of plates, and the temperature of water and steam within the boilers. The plates of a Cornish boiler are never less than $\frac{1}{2}$ an inch thick, those of a low pressure boiler usually $\frac{1}{4}$ to $\frac{5}{16}$ of an inch. A much larger extent of surface is consequently necessary in order to transmit a given quantity of heat, in a given time, through the thicker than the thinner material without injury; more particularly when the contained water, which abstracts the heat, is at a temperature of not less than 300° in the one, and not more than 220° in the other. The Cornish practice shews an intimate perception and knowledge of the advantages arising from this extent of surface, though I cannot find that the subject has ever been treated of by Cornish authors, which is the more to be regretted as an infinite variety of experiments have been made in Cornwall on the proportions of grates to the combustion; on the arrangement and proportions of the internal and external surfaces exposed to heat; on the mode of setting the boilers, introducing the feed, &c. So habituated also are the operative engineers and firemen to a comparison of the work done by the engine with the fuel consumed, that they are thoroughly versed in their business as an *art*, their skill being subject to strict and constant test by the daily registration

requiring stays to enable it to resist the small amount of internal pressure to which it is usually subjected. It is very susceptible of injury from collapse, if abundant provision be not made to supply it with air in the event of a vacuum. It is much affected by incrustation from deposit falling on the parts exposed to the greatest heat, causing leakage and frequent repairs, which, with the wear and tear of brickwork, occasion this kind of boiler to be much more costly in its maintenance than the Cornish.

The comparison of the Warwick with the Cornish results shews that in the former,

The rate of combustion is $\frac{1}{7}$th more rapid per boiler, and per square foot of grate per hour.

The rate of evaporation is $6\frac{1}{2}$ times more rapid from equal surfaces.

The loss of heat (the Cornish being unity) $12\frac{3}{4}$ per cent.

The results of the mean of the eight other experiments on the wagon boiler compared with the Cornish, show that in the former,

The rate of combustion is $2\frac{6}{10}$ times more rapid per boiler, and 3 times more rapid per square foot of grate per hour.

The rate of evaporation is 7 times more rapid from equal surfaces.

The loss of heat (the Cornish being unity) $24\frac{1}{2}$ per cent.

In contrasting the Warwick with the Cornish results, there are two corrections to be made before the experiments can be put on equal terms. Firstly, in the Warwick the whole of the smoke was inflamed, the practical evaporative value of which, from the experiments related in my former paper, appears to be about 12 per cent. In the Cornish the smoke passed off unconsumed; the result of the Warwick experiments must consequently be diminished 12 per cent., or the Cornish raised in that amount. This would cause the real difference in favour of the latter to be about 25 per cent. But there is a second correction to be made.

The result of my experiments at Warwick, during six months' continuance, is $18\frac{1}{2}$ cubic feet of water evaporated by 112 lbs. of coal from 212°, all waste and loss in the raking and banking up of the fires at night included. The Cornish, especially those at the United Mines, are also experiments of long continuance, waste and loss included, but as those engines work night and day, there is no loss arising from banking up the fires, or from radiation during the night. A diminution of the weight of coal used at Warwick is therefore required, equal to that burnt during the night and for raising the steam each

being composed of plates of $\frac{1}{2}$ inch and $\frac{1}{4}$ inch thickness, and transmitting heat respectively to water at 300° and 218°.

Notwithstanding also the near identity of the principal forces which seem to affect the evaporative product, there are differences in these two experiments among the minor causes in operation, which may have had an influence over the results. I allude to the proportions of the surfaces exposed to the radiating action of the fire compared with the weight of coal burnt in a given time, and with the surfaces exposed to what is commonly termed communicative heat. There is a near equality between the coal burnt and the radiant surface in the two experiments, but the difference is somewhat greater between the communicative areas, than between the total amounts of heated surface. Definite experiments are again wanting to determine the respective value of these portions of a boiler; but it will appear from some future remarks on this subject, that their indications are entitled to little confidence as general rules, excepting when the practice is precisely conformable to the particular experiment *.

By comparison with the Warwick, the result of the mean of the eight other experiments on the wagon boiler shews,—

The rate of combustion per boiler to be $2\frac{3}{10}$ times more rapid, and $2\frac{1}{10}$ times more rapid per square foot of grate per hour.

The rate of evaporation from equal surfaces $\frac{1}{8}$th more rapid.

The loss of heat (the Warwick being unity) $12\frac{1}{2}$ per cent.

This loss would amount to at least 25 per cent. on the mean if the VIIth, IXth, Xth, and XIIIth experiments in the Table, which were made on the Warwick system, and fully explained in my former paper, were excluded, and the comparison strictly made with those boilers only whose proportions of grates and method of practice were different. I have already contrasted the rationale of the Warwick and Cornish, as the former occupied an intermediate rank between the latter and the mean of all the others; and as I am not advocating my own or any other particular system, but endeavour merely to ascertain, by a rigid examination of the conditions of a series of experiments entitled to entire confidence, the causes which have produced various and particular effects, (and the experiments would be nothing worth did they not lead to some such useful end,) I have thought that end more likely to be attained by striking a mean from a large number of experiments on the same class of boiler treated on different plans. But no one can cast his eye over the Table without perceiving very marked differences in the ratios of the several parts of the wagon boilers under review,

* See pages 29 and 30.

as also in their evaporative products, which vary as much as 65 per cent. Though, therefore, the more active causes which influence the evaporative economy of steam boilers develop themselves prominently and decidedly by comparing three distinct and widely different systems of generating steam, as indicated by the Cornish, wagon, and locomotive practice, much instruction is derivable from the mutual examination of boilers of the same class, particularly when a change in the effect has been produced by some single change in the conditions of an experiment on the same boiler.

The Table presents several such on the wagon boilers. The VIth and VIIth experiments were made on the same boiler with one change; in the VIIth the grate was enlarged $\frac{1}{3}$th, but the whole capacity of the furnace about $\frac{1}{2}$, fired in each case in the same manner, and doing nearly equal work in the same time. The result was an economy of 50 per cent.

The effect of this simple alteration is clearly traceable through all the columns. The rate of combustion per square foot of grate fell from $11\frac{1}{2}$ lbs. to 8 lbs. per hour, and the proportion of surface to absorb the heat was consequently greater: in fact, the combustion was more perfect, and more time was given to transmit its product. The evaporative power of the boiler was also increased by the same causes.

Experiments VIII. and IX., at another place, with a boiler of very different proportions, furnish instructive facts. No alteration was made in the grate of this boiler, as it then appeared to me of a good proportion, though I should now somewhat enlarge it. The difference between the two experiments amounting to about 14 per cent., was simply caused by the more perfect combustion of the fuel in the IXth from a thickened mass of fire and the more perfect inflammation of its gaseous products, accompanied by a diminished rate of combustion, and a somewhat increased evaporative power as well as economy.

As this boiler was attached to a chimney of remarkable draft, I determined on making an experiment to ascertain the utmost amount of evaporation producible from the boiler. Every thing remaining the same, Experiment X. was undertaken, and to guard against any loss of heat by opening the fire doors during the process, the water was first brought up to the boiling point, the few hot coals on the grate were raked into a small heap on its centre, and the raw coal, amounting to about 2400 lbs., was then shovelled as quickly as possible into the furnace, until it was crammed quite full; the fire doors were then hermetically closed by filling the mouth-piece with well wetted

sleck. The damper for the first hour was kept almost close down, and when the regular rate of evaporation exhibited itself, which indicated—together with the vivid and uniform reflexion of the fire in the ash-pit—that the bars were well covered with ignited coal, the damper was very gradually raised, till the whole force of the draft was given to the furnace. The evaporation then became excessively rapid, and at the end of the fifth hour the fire was examined, that no aperture might be left for uncombined air to pass through it. The remainder of the coal was then thrown on, amounting to about 238 lbs., and the experiment continued till the whole was consumed, the damper being again gradually lowered as the thickness of fuel diminished.

One result rather surprised me, which was, that notwithstanding the rapidity of the current, and the certainty that the heat must have quitted the boiler in much greater quantity, and at a much higher temperature than in the IXth experiment, a very slight difference appeared in the weight of water evaporated by each pound of coal. The experimenters judged that two-thirds of the coal burnt and of water evaporated was accomplished in one half of the whole time, or in four hours; which would be, as regards combustion, at the rate of 15 lbs. per square foot of grate per hour. The ordinary working rate of that boiler was about 9 lbs., and from the appearance of intenseness in the fire and other indications, it was clearly perceptible that the rate of 15 lbs. per hour per square foot of grate was far too rapid a combustion for any boiler to sustain long without injury, particularly if supplied with water of bad quality. A practised eye is not a bad pyrometer, and without the aid of the latter instrument it was no presumption to conclude that such intensity of heat must speedily affect both the safety and durability of the vessel.

The almost inappreciable difference in the economic results of these last two experiments might seem to favour the opinion that very rapid combustion is not unfavourable to economy. It is, however, necessary to keep in mind that the experiment was conducted with precautions which cannot be carried into practice. An experiment of short duration was afterwards tried on the same boiler by attempting to burn about three hundred weight of coal per hour, or the same rate of 15 lbs. per square foot of grate per hour, with an even and regular fire; but it was accomplished with difficulty, with great waste from being obliged to widen the air spaces between the bars, and with great loss in evaporative product.

of the grates, which reduced the rate of combustion to about $6\frac{1}{4}$ lbs. per square foot of grate per hour. The Albion Mills' boiler, notwithstanding the swift combustion of the fuel, evaporated nearly as much water as the best of the other wagon boilers in the Table, the Warwick excepted. I consider this isolated exception to the results indicated by all the other experiments as regards the effect of the rate of combustion, to have been modified by the peculiar structure of the boiler; viz., by its presenting, as shewn in column 11, a much larger surface of internal flue, in proportion to that of the whole boiler, which compensated, by its superior efficiency in the transmission of heat to the water over an equal area of side flue, for the loss which could not but have resulted, had the same area consisted only of external surface. In this respect that boiler approximated more nearly than any of the other wagon boilers to one of the best properties of the Cornish construction, more than the half of whose heated surface is entirely surrounded by water; and it will be seen that its product considerably exceeded that of the London boilers, which had no internal flue, and nearly equalled the performance of the Clithero and Preston boilers which had internal flues, and whose rates of evaporation from equal areas were nearly identical with that at the Albion Mills. In like manner, the absence of internal flue in the London boilers seems to account in some degree for their inferior performance as compared with the Clithero and Preston experiments. Though, therefore, in assigning to boilers a certain power of generating steam from some certain area exposed to heat, it is necessary to discriminate between the vaporizing value of the arrangement of the surfaces; of which at present we may be called entirely ignorant.

The absence of internal flues in the Warwick boilers was compensated, or much modified, by the peculiar treatment of the fuel, and the diminished rate of combustion previously described; but there can be no doubt, by the comparison of parts and results with the Cornish, that the evaporation would have been effected with still less fuel from the former, had those boilers been supplied with internal flues.

These indications of the greater efficiency of interior over exterior surfaces, point to the probability that equal areas of the marine boiler would exceed, in evaporative value, equal areas of the boilers under review, provided the element *time*, and *intensity of calorific action*, as hereafter to be explained, be judiciously incorporated with the practice. A knowledge of the evaporative economy and power of the marine boiler would greatly tend to enlighten us

on the respective value of surfaces exposed to heat within and without the water.

All the foregoing conclusions are liable to a certain amount of error from the want of identity in the coals employed in the different experiments, not as regards the chief elements affecting evaporative economy, but as respects the mutual comparison of results from boilers of the same class. There is, however, a curious fact yet to be pointed out, which will shew these observations and deductions to be free from any gross error occasioned by the use of a variety of coals in the different experiments.

By comparing experiments VI., VIII., and XII., which were conducted according to the usual practice at the three establishments with all possible care and skill on the common plan of firing, and with three varieties of coal, it appears that there was a difference of 13 per cent. between the greatest and least result, which would have been attributed to the varying strength of the coals; but when these same coals were treated under the same boilers, as in experiments VII., IX., and XIII. by a different manner, viz., by a slower rate of combustion, &c., the gross difference is only 7 per cent., and the product between two out of the three experiments is identical. The attentive mind will also see good reason to conclude, by inspecting the peculiar properties of the boilers experimented upon, that even this slight difference of 7 per cent. is attributable to other causes rather than to the fuel*. A true estimate of the practical value of varieties of coal can, therefore, only be formed by treating them in such a way as to cause the development of their full force; and the cheaper coals—cheaper, generally, because less fitted for house fires and delicate purposes—do often, in reality, contain the greatest strength of combustible matter, though mixed with impurities. I tried at Warwick the Newcastle, (Wylam Moor,) the Wednesbury, and the Netherton coals, which gave me, within a fraction, the same result; though Watt and other experimenters assign very varying strengths to them, and the last were the cheapest in price by one half over the Wednesbury, for engine purposes. On the other hand, it not unfrequently happens that a small difference in price is more than compensated by an increased effect from dearer coal. Nor will the same system of treatment invariably succeed. I have been occasionally defeated in every attempt to burn certain sorts of coal on my plan of thick fires, on extensive grates with slow combustion. In one instance, in Lancashire, the coal contained so much tar

* The difference or loss of 7 per cent. is found in that boiler which had no internal flue.

as to run in streams through the bars, and catch fire in the ash-pit; and in Glasgow certain coals defied the system altogether; these were rare instances, but ought to be cited.

If the statement of the fuel burnt and water evaporated by Smeaton's boilers, Experiment XIV., be correctly given by Mr. Farey, the product was not only very good for the period, but is larger than is usually obtained from an equal extent of surface at the present day. The combustion was very rapid, its rate being $20\frac{1}{3}$ lbs. per hour per square foot of grate. The evaporation 8.37 lbs. from 212° by one pound of coal. The dimension of surface exposed to radiant heat not being given, I have assigned to it in the Table the half of the bottom of these circular or *hay-stack* boilers, (as they are commonly termed in Staffordshire, where they are still much used,) having found the practice to be such generally, the bridge wall being built across the centre of the boiler, sloped down to the grate at the sides and ends of the bars. The circular is a good form of boiler for strength; equally bad with the wagon as regards incrustation; but perhaps better adapted for outdoor fixing, as it admits of being more advantageously covered, without a roof, to throw off the rain from the top and walls; but the wagon boiler has so many advantages over it for house fixing, besides admitting of internal flues, and other desiderata, that the use of the circular has long since yielded to the wagon form in the best appointed establishments.

Thus Smeaton's evaporative practice in 1772 appears, from the account extracted from Mr. Farey's work, (which I cannot, however, find in the fourth edition of Smeaton's Reports, 1812, referred to by the latter,) to be, as respects the Cornish of the present day, as 8.37 to 11.82, shewing a gain, since that period, of $41\frac{1}{4}$ per cent. only; an important economy certainly, but miserably small when compared with the strides made in the economy of steam.

Are we then to conclude that our methods of generating heat and steam, and of constructing evaporative vessels, have attained the utmost perfection which the strict laws of nature, and the limited ingenuity of man, forbid us from passing? Or, are we to hope that our knowledge of the theory of combustion, of the most profitable modes of treating combustible bodies, and of applying the heat elicited from them to the vaporization of water, are alike imperfect and alike susceptible of improvement? My own opinion inclines to the more cheering view of the subject, perceiving as I do many sources of waste which may be turned to profit, and believing that combustion, as a science and practical art, has not yet

received that attention which it merits, and whence any very marked augmentation of calorific economy must originate. Numberless as have been the variations in the structure and dimensions of steam-boilers, it appears that the efforts of improvers have been confined within the range of a very small circle; changes have been rung on the same or similar contrivances, but nothing has emerged which at present authorizes any wide departure in practice from long established forms, or which can be characterized as bearing the stamp of inventive genius. Experience, nevertheless, informs us that great diversity exists in the economy of our present practice; the comparative investigation of numerous examples should, therefore, throw a strong light on those principles which are most influential over the results, and it should indicate, with a near approach to certainty, that practice which is most likely to assimilate the product from one boiler with the product from another.

As boilers of the Cornish and wagon forms are by far the most numerous in use, it may be here most convenient, and before entering on the examination of the *locomotive*, to allude to those questions which the intelligent employer of a boiler frequently propounds, and desires to solve, viz. the best proportion of parts, and the best practice. These problems he can now readily solve for himself, by an attentive study of the facts placed before him in the Table.

Does he wish to fix a new boiler? The Cornish points itself out as the best known; and having decided on the quantity of steam he requires in a given time, either for an engine or other purpose, he is furnished with a means of determining the weight of fuel which will generate it, *by rigidly adopting the measures of surface and proportions of parts which have given relative effects.*

Does he desire to ascertain if his present practice be good or defective? As regards the rate of combustion, the simple determination of the weight of coal he is consuming per hour, divided by the area of his grate, will decide that question; and by comparing that product with the entire surface presented to absorb it, he will, if his boiler be well set, arrive at an approximation to the amount of water evaporated on consulting the Table; but no absolute and sufficient knowledge of the economy of his practice is attainable without ascertaining the weight of water vaporized. No one can tell him this fact: *it is the work which his boiler* is doing, and it can only be determined by experiment.

Does he find, after this trial of the evaporation, that an equal surface has produced, from other boilers of the same form, a greater result? He must compare the area of his grate, and the rate of combustion of the fuel with the best parallel examples in the Table, and bring his practice to a conformity with

them if they disagree; this will inform him of the comparative calorific value of the fuel he is using: if he still finds that he is deficient in boiler-power, the only cure is to add more. If circumstances prevent him from so doing, increase of effect and of economy will generally follow the enlargement of a grate to such dimensions as a fireman can skilfully command, the width of which is only limited by that of the boiler, and the length not much exceeding six feet*. There is evidently a considerable range permissible in the rate of combustion, without much loss of effect, or much evil arising to the boiler; but it is also evident that the slowest rate is the best on every account, and that is the point to be aimed at achieving. The combustion should not exceed 7 lbs. per square foot of grate per hour; but it will be the true interest both of the engineer and employer so to proportion parts and practice as to bring the rate of combustion at least as low as 4 lbs. per square foot of grate per hour; and the Table shows that the greatest effect was produced when the boilers were so proportioned as to bring down the combustion to less than 3 lbs. per square foot of grate per hour.

With respect to the setting of a boiler, every respectable engine maker is acquainted with the best methods; and from such persons, or from good examples, the employer of a boiler should take his plans, and not permit so important an element in the final economy of his practice to be left to the discretion of a bricklayer, as is too frequently the case.

But little information, either of scientific or practical value, on the important

* To determine this point satisfactorily to myself, I used, in my early experiments, a *moveable bridge*, commencing with a grate 10 feet in length. The evaporation was noted each day, without which I considered every experiment as valueless. This bridge was traversed a foot at a time nearer to the fire door. The best effect was produced with bars 5 feet 6 inches to 6 feet long, under the Warwick boilers, as per Table I. I found thickness of fuel far more economical than an *excessive* extent of grate surface. No fireman can spread fuel accurately beyond 8 feet from the fire door, which is about the extreme length of a 6 foot grate, including the dead-plate of the furnace mouth. I also found, that beyond 7 feet of bar a sufficient supply of air did not reach the end; that the fuel burnt dull and uselessly; and no doubt much of the air which did enter, passed through the half ignited coals, not only unproductively as an element of combustion, but wastefully, by mixing with and cooling the stream of heat.

With a rate of combustion as slow as 4 lbs. per square foot of grate per hour, a grate (were it practicable) should resemble a fine sieve. My bars, *for this rate of combustion*, were fixed, after numerous trials, at $1\frac{1}{2}$ inch broad on the upper face, and the air spaces between the bars at $\frac{1}{4}$ of an inch wide; consequently, no *cinders* fell through into the ash-pit, the refuse being *ashes*, and utterly worthless. My fireman usually sat in the ash-pit to take his meals, and though he had half a ton of coals on fire above his head, he feared no fall of hot coal, as the poker was never used, nor was the fire door opened but once, after charging, during the entire day.

subjects of steam-generators, combustion, and evaporation, is to be obtained from the numerous works and treatises on the steam-engine. Mr. Farey's volume is chiefly a history of the early inventions and practice; it contains numerous and minute details, and is useful as a record of dimensions and strength of parts. It is to be regretted that the writings of many popular authors abound too much in formulæ and hypotheses, too little in facts: for, unfortunately, the detection of fallacies in particular statements tends to diminish confidence in their general accuracy. It is, however, necessary for the sake of truth—the basis of science—that errors which have crept into works of the highest estimation should be pointed out. Of these several are to be found, as regards the subjects under consideration, in the splendid edition of Tredgold, by Woolhouse, which may be traced (and the same remarks apply with equal force to other authors) to a limited knowledge of ascertained facts on the large scale of practice; to an undue reliance on experiments conducted in the laboratory, or of short duration; to conclusions drawn from the practice of some one engineer, or deduced from the results of some particular class of boiler.

At page 112, a Table is given by Tredgold of the calorific powers of a great variety of combustible substances; and we are told, that 8.22 lbs. of "caking coal" and 9 lbs. of "coke prepared in close vessels," are requisite to convert a cubic foot of water into steam. "These quantities," says the author, (page 113,) "derived entirely from theoretical considerations, are so near to the actual effects obtained in practice, that they shew us we have little to expect in the form of improvement," &c.

Fortunately, the Cornish and other engineers, being more habituated to practical than to "*theoretical considerations*," have thought otherwise, and we happily know that this theoretical barrier has been passed by nearly fifty per cent.; and there is abundant reason for hope that no barrier exists against obtaining still higher results.

He again tells us, page 116, that "one foot area of grate, for each horse power, is the common rule of practical engineers." I can find no such agreement in the practice of engine makers, which is as various as their number is large; the practicable size of a grate depends very much on the width of the boiler, and the widths of boilers on the fancy of their makers.

A rule is also given, pages 115 and 116, for the width of air space between the grate bars, and much learning is exhibited in determining the coincidence of results, as flowing from hypothesis and formulæ, with the presumed practice. We are informed, that one-third the width of the fire-bar is a good

rule for the air space, without any reference to the rate of combustion, or to the peculiar nature of the fuel, and without perceiving, that upon the width of the air space materially depends the waste in the ash-pit, and other important consequences; and the rule leads to this absurdity, that a fire-bar three inches broad must have an inch air space; and one of an inch in width, an air space of one-third of an inch, and so on.

We are also informed, page 118, that for low-pressure boilers, the best effect from fuel will be obtained by 8.1 square feet to vaporize one cubic foot of water per hour, and 8.9 square feet for high-pressure boilers. The most experienced engineers in Lancashire (the practice of which county I quote, as I am better acquainted with it than that of any other part of England) allow at least from 12 to 15 square feet for low pressure, and the Cornish engineers from 60 to 70 square feet for high-pressure purposes, in order to obtain that effect as shewn in Table I., columns 46 and 47.

Smeaton's experiments on coal of several varieties, coke, wood, &c., are repeatedly cited, and pass from treatise to treatise as decisive, or at least illustrative of the respective calorific value of these combustibles. Smeaton's experiments were made on a boiler only $2\frac{1}{2}$ feet in diameter, and it requires no reasoning to shew that the grate and the setting which would suit one combustible, would be utterly unfitted to develope the powers of another; and that experiments, conducted on so diminutive a boiler, can give at best but deceptive indications of the effects of combustibles on the large scale of practice.

The name, experiments, and practice of Watt are also very commonly used by writers on the Steam Engine as arguments and data for empirical rules which Watt would have been the first to repudiate. This is a system which cannot be too strongly reprobated; it has had the effect of restricting rather than of extending the conquests and realm of science; and much injustice has been done to the character of these distinguished men by the excessive zeal of disciples. Smeaton and Watt were not merely engineers; they were great *practical philosophers;* their career was marked by a succession of bounds over mechanical obstacles; their writings exhibit a modesty becoming greatness; and in giving to the world an account of their experiments and discoveries, they never dreamed that they would be regarded as laws to control future ages, or be viewed otherwise than as progressive steps towards the attainment of a perfection in their favourite pursuits, which they well knew neither the life nor the labours of any single man (however great his genius) could accomplish.

LOCOMOTIVE BOILER.

EXPONENTS.

Column of Table.	lbs.	
48....	1	of coke burnt under one boiler in 6.45 seconds.
20....	79.33	of coke burnt on each square foot of grate per hour.
41....	12	of water evaporated by one square foot of heated surface per hour from 212°.
37....	7.21	of water evaporated by 1 lb. of *coke*, from 212°.
Calculated....	5.77	of water evaporated by 1 lb. of *coal* from 212°.
Mr. Wood....	5.17	of water evaporated by 1 lb. of *coal* from 212°.

The circumstances under which locomotive boilers are worked,—their dimensions, construction, strength, and nature of materials, are so widely dissimilar to every other species of evaporative vessel, that it would be vain to attempt to draw any other conclusion as to their merits than that which is furnished by their practical results. Strict analogy between them and others ceases with the production of heat, for such is the devouring nature of the locomotive engine, so short the time allowed for the heat to act on the surface of boiler exposed to it, and so limited the space assigned for generating steam, that the processes by which economy is sought at fixed engines would seem to be thought unattainable in the locomotive, and recourse is had to other contrivances. The generation of heat and steam in the locomotive is, however, governed by precisely the same laws as in other boilers, and the economical inferiority of that boiler must be owing either to the absence of some one or more of those elements which comparison shews chiefly to influence their economic results; or to the presence of some bad quality, from which other boilers are exempt. The decomposition of its parts and effects exhibited in the Table, enables us to ascertain in what respects the locomotive boiler and practice differ from that of the Cornish and wagon boilers, and to assign, with tolerable certainty, the true causes for their widely different evaporative economy. The comparison of Experiment XVI., (which is the mean of eleven experiments made by M. de Pambour on six engines on the Liverpool and Manchester Railway in 1834,) with the mean of the Cornish is, that

The rate of combustion per boiler is $6\frac{8}{10}$ times more rapid.

The rate of combustion per square foot of grate per hour, 23 times more rapid.

The rate of evaporation from equal surfaces, 12 times more rapid.

The loss of heat, (the Cornish being unity,) 51 per cent.

The comparison of the locomotive with the mean of the eight experiments on the wagon boilers, shews, that

The rate of combustion per boiler is $2\frac{1}{2}$ times more rapid.

The rate of combustion per square foot of grate per hour, is 7 times as rapid.

The rate of evaporation from equal surfaces, $\frac{7}{10}$ths more rapid.

The loss of heat, (the mean wagon being unity,) 27 per cent.

It would be mere waste of words to institute any more minute comparison between the locomotive and the Cornish boilers, than is given above.

It may not, however, be uninteresting, nor altogether useless, to shew how entirely the relation between boilers of equal surfaces is disturbed by a different practice as regards rapidity of combustion, and by a different arrangement and strength of parts and metal. The comparison between the locomotive and the mean of the eight wagon boiler experiments, is, for this purpose, as perfect as if a single boiler of each class had been constructed with the express intention of submitting the two vessels to the most rigid tests. By referring to the Table, it will be seen, that there is an almost perfect identity in the total, in the radiant, and in the communicative areas, between the mean of the eight experiments on the wagon, and that of the eleven experiments on the locomotive boiler. So perfect an analogy between the boilers, resulting too from such numerous experiments, renders the accuracy of the conclusions deducible from an analysis of the respective practice, unassailable by the most fastidious experimenter.

We observe, then, first, that a near equality exists in the entire surfaces exposed to absorb the heat—also, between the respective areas receiving the radiant and communicative caloric; but, comparing the relation between the areas of the grates, that of the wagon-boiler exceeds the locomotive in the ratio of $3\frac{7}{10}$ to 1; the same difference, therefore, obtains inversely between the ratios of the areas of the grates to the total surfaces. The locomotive boiler would thus necessarily have the advantage of presenting between three and four times greater surface to absorb the heat generated on the grate, if the rate of combustion, or the weight of fuel burnt on each square foot of grate in equal times, were alike in the two experiments. But this is not the case; double the weight of fuel is burnt on the grate of the locomotive, in the same time, as on that of the wagon boiler, which is $3\frac{7}{10}$ times larger, and the consequence is, that the rate of combustion in the locomotive is seven times more rapid, and that it offers only $\frac{4}{9}$ths the surface of the wagon boiler for the absorption of equal quantities of heat in equal times—or, in more accurate language, for the absorption of the *heat produced from equal weights of fuel in the same time.*

The equality, therefore, which seemingly exists between the two boilers is merely an equality of figures.

The result of this discordant practice on the two boilers is, as before shewn, a loss by the locomotive of $\frac{1}{3}$rd the value of that proportion of the entire heat of the fuel which has been realized by the wagon boiler. The rate of evaporation from equal surfaces is augmented in the locomotive by 65 per cent.; so that the attempt to increase what is termed *evaporative power*, by that amount, has been attended by a sacrifice of 33 per cent. of fuel. As regards the Cornish, we have also seen, that the attempt to increase the rate of evaporation 12 times, is accompanied by a loss in evaporative economy of 51 per cent.

The nature and strength of the locomotive surfaces form a striking contrast with the corresponding parts of the wagon boiler, and had not some antagonist quality of an overwhelming character influenced the result, the locomotive ought to have exhibited a performance greatly superior to that of the wagon boiler.

The radiant is to the communicative area of the locomotive as 1 to 7; in other words, the surface exposed to the direct action of the fire is $\frac{1}{8}$th, and that of the remainder $\frac{7}{8}$ths of the whole boiler, in which there is a near correspondence between it and the wagon boiler. The metal of the locomotive radiant surface may be called nearly twice as thick as that of the wagon boiler, which is unfavourable—but the communicative area being composed of tubes not exceeding one quarter or one third the thickness of the wagon boiler, infinitely more than restores the balance in favour of the locomotive; and though, as before observed, we have no definite knowledge of the heat-transmitting powers of metal of varying thicknesses, we do know that the times of transmission are greatly accelerated by employing thin substances, and the conclusion will not be disputed that if $\frac{7}{8}$ths of the surface of the wagon boiler were put upon equal terms with the locomotive by diminishing its thickness $\frac{2}{3}$ds or $\frac{3}{4}$ths, a very much larger portion of the entire heat would have been absorbed, and the economical result have been considerably augmented in the wagon boiler: and, conversely, we know that if the tubes of the locomotive consisted of metal three or four times as thick, the product in steam from equal amounts of heat, would have been very considerably diminished.

Another marked advantage in favour of the locomotive, arises from the distribution of the heat, through the subdivision of its volume among such numer-

ous thin tubes. This principle of breaking up the heat into fragments is evidently one of exceeding great consequence in the construction of boilers whose purpose requires that their dimensions and weight should be compressed into the smallest possible bulk.

The antagonist quality, which has exerted an influence over the result so powerful as not only to neutralize these superior properties of the locomotive, but to reduce their effect so far below that of the boiler compared with it, seems to be solely referable to the operation of the element *time*. The mean length traversed by the heat in the locomotives under review, was 7 feet, whilst that of the mean of the wagon boilers was 72½ feet, or a difference nearly of 10 to 1. The combustion of the first was at the rate of 9.3 lbs. of coke, and of the second at the rate of 3.6 lbs. of coal per minute. In a minute of time, therefore, the whole volume of the heat generated from 9.3 lbs. of coke, quitted the boiler after travelling only 7 feet, whilst the heat of 3.6 lbs. of coal did not escape till it had travelled 72½ feet; in other words, every particle or atom of heat remained 26½ times longer in operation on the surface of the wagon, than on that of the locomotive boiler.

Thus, on the score of economy of heat, the present disposition of the parts of a locomotive boiler is evidently unequal to contend with this powerful adversary; and certain it is that neither the quality, economy, nor durability can ever be engrafted on boilers of the present construction and proportion of parts, with the existing practice; and it is equally certain that no mere change of form or proportion of parts would effect an improvement of much consequence until the element *time* be permitted, in some shape or other, to enter into locomotive practice as a reasonable ingredient in the generation of heat and steam. The locomotive boiler is a striking instance of what consummate constructive skill can effect in endeavouring to pass a boundary opposed alike by the constitution of metals, and by the warning results of all previous experience. Every sound principle is sacrificed to the rapid production of steam; steam is bought at the price of gold; and the absence of the quality durability, involves expenses unknown in other engines, from the practice of connecting almost every part of the engine with the boiler. If this intimate union of the boiler with the engine be unavoidable, or indispensable, the durability of the former ought to be a quality especially sought after. Economy in the production of steam, and durability of the vessel, are nearly synonymous terms; both qualities being obtained by a judicious disposition of parts, and by a judicious practice and manage-

ment. The safety of the vessel as regards explosions is also a property nearly allied to the two others, so far as the metal is affected by the heat. The locomotive is free from one bad quality, possessed in excess by wagon boilers, the precipitates from the water do not fall upon the parts exposed to the greatest heat. The tubes also seem to be weaker than the external cylinder which contains them, and generally also than the fire box, so that though these are continually bursting, no great harm ensues; nevertheless, the absence of many very violent explosions seems to be fairly attributable rather to the constant presence of the engine-man, and to the inadequacy of the boiler to its work when leakages denote weakness and the necessity of repair, than to any inherent principles of safety in the vessel itself. The rapid decay of the fire box and tubes of the locomotive boiler—uninfluenced as that decay is by the incrustation of deposit—affords us a definite and valuable fact, free from any disturbing considerations, which incontestably proves that heat may be supplied to a vessel containing water at such an intensity and in such quantity that its transmission cannot be effected, without speedily destroying the virtue of the metal; and the short-lived existence of these boilers in a sound and healthy condition proves that the temperature of the fire far exceeds the limit at which it can be applied with impunity. The metal of a condemned fire box frequently appears on examination to have lost all its original characteristics; there are no remains of fibrous or crystalline appearance; both copper and iron seem to be transmuted into a species of metallic cake, brittle, of no lustre, and yielding to the slightest blow.

For the sake of simplifying the Table, I have given only the mean of M. de Pambour's eleven experiments, but persons interested in locomotive boilers will not find their labour lost by reducing each experiment to the terms of the Table and submitting them to mutual examination. This will be rendered more apparent from the following notice of a very useful experiment by Mr. Robert Stephenson, which I feel impelled to make, as that experiment appears to me to have been greatly strained in its application by writers on railways.

With the view of obtaining a theoretic expression for the unit of heating surface, necessary to effect the requisite evaporation for the supply of a locomotive engine, M. de Pambour has used the experiment referred to, as detailed by Mr. Wood in his Treatise on Railways*, from which it would appear that

* Wood on Railways, 1st ed. page 403, and 3d ed. page 528.

the effects of the surfaces exposed to the radiant and communicative heat, stand to each other in the ratio of three to one. "If we admit (says M. de Pambour, page 179,) in consequence of the experiment related in our first chapter, that each unit of surface exposed to the communicative heat, produces a third part of the effect that same surface would produce if exposed to the radiating caloric, the heating surface above (viz., 41.47 square feet fire box, and 293.11 square feet tubes) may be represented by 139.24 square feet exposed to the immediate or radiating action of the fire; and as those 139.24 square feet have produced in an hour the evaporation of 55.32 cubic feet, we see that each square foot has evaporated a volume of water expressed by 0.401 cubic feet."

The conclusion here drawn is, however, inadmissible, and inconsistent with the facts clearly shewn by the mutual comparison of the surfaces of fire boxes, and tubes with the evaporation, in five out of the six engines which formed the subjects of M. de Pambour's own experiments. 1st. The Atlas's boiler has a fire box of 57, and the Vesta's one of 46 square feet, difference 11 square feet, which multiplied by 3 gives 33 square feet assumed to be its equivalent in tubes. This sum added to 218, the area of the Atlas's tubes, makes 251 square feet, which is the area of the Vesta's tubes within five feet; yet the Vesta with the smaller fire box exceeded the Atlas in evaporative economy by 11 per cent.! 2dly. The Vesta's fire box exceeds the Vulcan's by $11\frac{1}{2}$ square feet, which $\times$ 3, and added to the tubes, makes a total of 290 square feet, and brings the two boilers on the presumed equality as to surfaces within 10 feet; yet the Vulcan with a fire box less than the Vesta's by 25 per cent., exceeded the Vesta in evaporative economy, by 24 per cent.! 3dly. In like manner, the Vulcan, whose fire box was less than that of the Atlas by 39 per cent., exceeded the latter in economy by 46 per cent.! 4thly. The Leeds, of precisely the same proportions as the Vulcan, gave similar results! And 5thly. The Fury, whose fire box was somewhat less than that of the Leeds and Vulcan, having tubes coinciding in surface with them, also greatly exceeds both the Atlas and Vesta, though it is a little, and but little, inferior to the Vulcan! The Firefly I exclude from comparison, (though it proves the same fact,) as that engine and boiler were not thought to be in very perfect order.

Were the deduction as to the relative evaporative value of the two surfaces correct, the present complex boiler may be discarded, and a simple fire box having a superficies of 140 square feet might be substituted with the certainty of producing equal effects. It does not, however, I think, require the spirit of

prophecy to say that such an arrangement of parts would altogether fail in vaporizing the required weight of water. To effect it from the reduced surface the grate would require to be doubled or trebled in size; and from what has been noticed of the loss traceable to the short distance travelled by the heat, and its short duration about the boiler, it must be quite evident that an arrangement of parts founded on the conclusion drawn by these writers from Mr. Stephenson's experiment, would end in the defeat of their expectations and be accompanied by an enormous waste of fuel. Mr. Stephenson, in stating his experiment originally, seems to have been perfectly aware of its limited nature, and to have cautiously refrained from theorising beyond the facts disclosed by it.—That experiment is instructive so far as it goes, and like effects would have resulted from like surfaces, under similar conditions; but the conditions of the boiler under examination, compared with that operated upon by Mr. Stephenson, are very dissimilar, and have undergone several changes, any one of which would have destroyed identity between the boilers, had such identity even originally existed. The relative areas of the fire boxes and tubes were very different, the ratio between those parts being as 4 to 1 in Mr. Stephenson's, and 7 to 1 in M. de Pambour's boilers. Neither was the experiment made with a blast into the chimney, and there is every reason to conclude that with a greatly increased rate of combustion, which the blast would occasion, a very different proportionate vaporization would have taken place in the fire box and tube compartment of the experimental boiler; and a still greater difference in the respective products of the two compartments would arise from a variation in the area of grate as compared with the two surfaces. The theoretic expression for unit of surface would have been perfectly exact and equally sufficient for M. de Pambour's purpose, by stating that a volume of water expressed by 0.165 cubic feet was evaporated per hour by 1 square foot of surface, which surface consisted of $\frac{1}{8}$th exposed to radiating and $\frac{7}{8}$ths to communicative heat.

We thus see how hazardous it is to quit a fact, for the sake of a neat expression.

The results which I have just stated, as flowing from a strict comparison of the respective surfaces, with the respective evaporative economy, of 5 out of the 6 engines operated upon by M. de Pambour, were to be expected, and might have been foretold; as a larger proportionate area to absorb the heat generated in and escaping from the fire-boxes, allots to each particle of heated matter an increased time to dispose of its caloric; and this conformity with a law which would seem self-evident, and which is seen to pervade

all the other experiments, is satisfactory as tending to confirm our reliance on the care and accuracy bestowed by M. de Pambour on the conduct of his highly valuable experiments; nor would this able experimenter and analyst have fallen into an error of reasoning confuted by his own results, had his attention been equally given to the phenomena of heat, instead of being confined to facts which regarded the engine alone.

Dependent on the proportions of fire box to tubes in the locomotive boiler, other important consequences require notice. The intensity of the heat will be greater in those boilers which present the smallest area to receive the radiating caloric from each pound of coal in an unit of time, than in those exposing a larger area; thus, it seems difficult to combine in the locomotive the greatest sum of economy, with the greatest sum of durability—the dimensions of the boiler being a limited, and the required product of steam a fixed quantity.

In locomotive boilers of equal lengths, and having an equal number of tubes, a larger fire box and grate are necessarily accompanied by a diminished length of tubes; from which change of dimensions four results flow: 1st, a larger area of grate in proportion to the whole amount of fuel burnt upon it; 2ndly, a larger area of radiant surface to absorb the heat of each pound of fuel; 3rdly, a smaller proportionate area to absorb the communicative heat from equal weights of fuel burnt; 4thly, a shorter distance travelled by the heat, and a shorter duration of it in the tube compartment.—It has been shewn that, in economy of fuel, the five engines ranked as follows: Leeds, Vulcan, Fury, Vesta, Atlas; being in the inverse order of the areas of their fire boxes; the fuel consumed per *square foot of fire box per hour*, being in the same order, (with one exception,) viz. 16.03 lbs., 17.11 lbs., 15.31 lbs., 13.55 lbs., 10.58 lbs., a result greatly favouring the durability of the larger fire boxes, whilst the loss in evaporative economy is as clearly traceable to an increased deficiency in the element *time* for the absorption of the caloric in the tube compartment.

These conclusions might seem to militate against the results and previous reasonings derived from enlarged grates and extended surfaces, exposed to the radiating caloric in other boilers. A little examination and reflection will, however, dissipate the apparent contradiction.

I stated, at the commencement of this investigation of the properties of the locomotive boiler, that strict analogy between it and others ceases with the production of heat. In all boilers supplying steam to fixed engines, the steam consumed by the engine—which is a determinate quantity—governs the rate

of its generation in the boiler, and space permits the adoption and use of boilers of such dimensions as to endow them with a *power* of generating both heat and steam greatly in excess over the demands of the engine. This *surplus power of boiler* permits the employment of the controlling agency of the damper, which not only regulates the generation of the heat, but also detains that heat about the boiler, and times its departure from it.

The object of the locomotive engine is speed. Its tractive power has two limits; the force of adhesion to the rails, and the supply of steam. Its maximum load being determined by the amount of adhesive force, the mechanical power of the engine depends on the pressure of the steam; and the velocity of the engine depends on the quantity of steam, at that pressure, which the boiler can furnish in a given time. Hence it is that the power of a locomotive engine has been said by writers on the subject *to reside in the boiler;* and so long as the boiler is inadequate to supply any more steam than is barely sufficient to overcome the load determined by the adhesive force, or smaller loads at higher velocities, the expression is true in fact, though it appears to me to be neither a logical nor a philosophical expression, as it is only true so long as the evaporative power of the boiler is insufficient for, or merely balances the demands of the engine. Supposing the boiler to possess a power of furnishing steam to the engine in excess over its demands, the expression ceases to apply; and until the boilers of locomotive and of all other engines do possess this *power in excess,* they must inevitably continue to be prodigal of heat; for it is only through *an excess in the power of generating heat and steam over the demands of the engine,* that we can economize fuel; it is only by this *surplus power* that sufficient *time* can be allowed for each particle of heated matter to transmit its burthen of caloric to the water, before it quits the boiler.

It will be observed, that the Table exhibits the particulars of the locomotive combustion, in M. de Pambour's experiments, *coke* being the combustible, which has been compared with that from *coal* in all the other boilers. I have extracted from Mr. Wood's third edition of his Work on Railways, page 333, his own experiments on evaporation from a locomotive, *coal* being the combustible, in an engine termed by him the "improved Killingworth," all the elements of which will be found decomposed, Experiment XVI. These, so far as I can ascertain, are the only recorded facts of locomotive evaporation with coal as fuel; and though the boiler of the "improved Killingworth engine" is evidently one not fitted for railway engines in general, (nor is it recommended

as such by Mr. Wood,) yet the facts, coming from so respectable a source, are entitled to credence, and are highly useful as illustrating the calorific value of coke and coal as combustibles in the locomotive.

Prodigal as the other locomotives have seemed to be, this boiler exhibits an appetite for fuel which a near proximity to a coal-pit alone could satisfy; nevertheless, its voracity does not appear to me to be much if at all greater than other locomotive boilers, whose *apparent* evaporation and economy are superior, chiefly because they are supplied with the stronger and more condensed food of coke. In my former paper I stated the necessity of diminishing the results of evaporation obtained from coke, in the locomotive boilers, or raising those from coal in the other boilers, that the comparison between them might be made on equal terms. Mr. Wood's experiments on the "improved Killingworth boiler" tend to confirm me in not having over-rated the strength of coke in my former estimate, as the evaporation obtained by him is 4.4 lbs. of water by 1 lb. of coal, and that from coke 6.2 lbs. in the other engines, (from the temperature of the water in the tender,) being a difference in favour of coke of 40 per cent.—Though Mr. Wood's boiler, from its dimensions and proportions of parts, would be considered inferior in its economic qualities, compared with the other locomotive boilers examined, I do not think it much so, and it possesses some advantages which the others have not; and, though I find M. de Pambour, page 349, quoting from Mr. Wood, that "a ton of coals of good quality produces a little more evaporation than the same weight of good coke," I feel fortified by the positive fact now adduced from experiments by Mr. Wood himself, in adhering to my former conclusion, and I have, consequently, in all the foregoing comparisons, reduced the evaporative product obtained from the locomotives by 20 per cent., leaving the precise facts of the experiments in the Table*.

* When writing my paper of last session, I was not aware that any doubt of the fact of the superior strength of coke over coal existed; and seeing that every day practice and experience in various arts, and under steam-boilers also, verified my own opinion, the only question in my mind was the degree of difference in calorific value of the two combustibles, and I contented myself by assigning $\frac{1}{5}$th in favour of coke, which I then thought, and still think, too low. It seems that Smeaton found the effect of coke to be $\frac{5}{6}$ths that of an equal weight of the same coals from which the coke was made—66 lbs. of coke being obtained from 100 lbs. of coal, (Farey on the Steam-Engine, page 172,) but it does not appear that he made any alteration in his grate to suit it for coke, and without such adaptation the experiment tells nothing. I have found, by proper management, 75 lbs. of coke equal in effect to 100 lbs. of the self-same coal

AN INVESTIGATION

OF

THE RELATIVE TIME DURING WHICH THE PRODUCTS OF COMBUSTION, FROM EQUAL WEIGHTS OF FUEL, CONTINUE IN OPERATION ON EQUAL AREAS OF THE SURFACE OF THE BOILERS; WITH AN ESTIMATE OF THE QUANTITY AND INTENSITY OF THE HEAT APPLIED TO THEM.

The structure of the parts, and the mode of setting a boiler, occasion the heat applied to it to travel greater or less distances, and to pass over very unequal extents of surface, in equal or unequal times. The distance travelled I shall consider as determined by the length of the circuit which the heat is compelled to traverse from the grate till it quits the boiler. The time in which it performs the circuit, is the period of the duration of a particle of heat about the boiler, and is the first question to be considered.

from which the coke was made, by actual practice on many tons during several weeks of work. Mr. Pellatt's mode of burning coke exhibits, in a far more perfect manner than any steam-boiler can do, the relative calorific value of coke and coal. The space within his glass pot furnace gives abundant room for the combination of air with the gaseous products; the flames are not extinguished by comparatively cold surfaces like those of a boiler, which, after inflammation, reduce them back again into smoke; the heat requisite for perfect combustion is always present; and his furnaces are particularly favourable to the development of all the power of coal; yet he finds common gas coke to be superior to coals in heating power by 25 per cent.; and gas coke is stated by M. de Pambour to be found inferior to Worsley coke by 12½ per cent., which no one acquainted with coke will doubt—thus exhibiting an excess over coal by 37½ per cent. from good oven coke. Coke may be defined to be the concentrated essence of coal; it has been deprived of its weaker calorific elements; it consists chiefly of carbonaceous matter, and must, necessarily, give out a much greater quantity of heat than an equal weight of coal. If the results of locomotive practice be such as have been presented by M. de Pambour and Mr. Wood, (though not confirmed by the latter gentleman's experiments above quoted,) they merely shew that a locomotive furnace and boiler are ill adapted to develope the power either of coke or coal; and they simply prove, that in the way in which coke and coal are submitted to combustion, and their heat to absorption in that boiler, the loss on the whole heat generated is proportionally much greater on coke than coal. The products of the combustion of coke (unincumbered with the caliginous particles attending that of coal, and occupying much less space than flame) escape more rapidly and freely through the tubes than the more gaseous and voluminous products of coal, from which they have previously been purified. That the heat from coke is more intense than that from coal is a matter of notoriety, and the fact (if it be a fact) of coal having produced as great an effect as coke in locomotives probably arises from the longer detention of the flame and heat of the former within the boiler, because, from its greater volume and expansion *it cannot get out* as fast as the heat of coke, which may have led to the hasty and fallacious conclusion, that coke is the weaker combustible.

The rate of combustion, or the time in which a pound of fuel is burnt, seems to me to be the best practical measure of the velocity of the products of that combustion about a boiler. The mind readily apprehends, that if a pound of coal be consumed under one boiler, in half the time that it is consumed under another, the velocity of the current must be twice as rapid in the one case as in the other; but if the velocity be expressed in feet per minute, or miles per hour, no information is conveyed of an appreciable or practical nature; nor does that expression reach the source or origin of the current, viz., the rate of combustion. The real velocity of the current of heat is not determinable from this datum of the rate of combustion, and it is very different in different parts of the same boiler, nor can it be accurately ascertained without some fitting instrument; and all that has been written about the density and rate of motion of the heat in a locomotive boiler—based on calculations of its velocity from that of the steam escaping from the blast pipe—strikes me as utterly worthless in a practical sense, as giving no knowledge, and leading to no useful end; and such calculations would, I am convinced, be found very wide of the truth if tested by a meter *.

I shall simply attempt to determine the relative, not the positive, times occupied by the heat in operating on the different boilers under review, and state the result in terms of the ratios which these times bear to each other. In endeavouring, therefore, to estimate the value of *time* as an element influencing evaporative results, I shall consider it to be referable—

1st. To the rate of combustion.

2d. To the distance passed over by the products of combustion before they quit the boiler.

3d. To the time in which the heat traverses the boilers.

4th. To the period of the duration of the heat about equal areas of surface.

It is necessary to state that the rate of combustion now spoken of, is not the rate reckoned on the square foot of grate, but the consumption of fuel in an unit of time under *one* boiler of each class. It has, therefore, been necessary to reduce the whole weight of fuel burnt in a given time, in the several experiments, to its equivalent under *one* boiler. The evaporative results are not affected thereby, but remain strictly comparable with the results arising out of the present investigation, and the facts which appear in Table II., as data required for this analysis, are all derived from particulars given in Table I.

* Wood on Railways. Third Edition, page 508.

TABLE II.

Rate of combustion in one Boiler.			Circuit of the Heat.	Heated surface.
Boilers.	Pounds burnt per minute.	Time in burning 1 lb.		
	lbs.	seconds.	ft.	sq. ft.
Cornish.....	1.361	44.08	155.00	961.66
Warwick.....	1.566	38.31	50.66	151.66
Mean Wagon..	3.620	16.57	72.50	342.81
Locomotive....	9.295	6.45	7.00	334.56

PROPOSITION 1. The velocities of the current of heated matter through each boiler, will be to one another directly as the rates of combustion, and inversely as the time in which equal weights of fuel are burnt. Using the latter measure, it appears that the respective velocities are,

Locomotive to the Cornish as 44.08 to 6.45, or 6.835 to 1.
Ditto Warwick 38.31 to 6.45, or 5.939 to 1.
Ditto Mean wagon 16.57 to 6.45, or 2.569 to 1.

PROPOSITION 2. The distances passed over by the heat before it quits the boiler, are to each other directly as the circuits of the boilers: thus the

Locomotive is to the Cornish as 7 to 155.00, or 1 to 22.142.
Ditto Warwick as 7 to 50.66, or 1 to 7.237.
Ditto Mean wagon as 7 to 72.50, or 1 to 10.357.

PROPOSITION 3. The times in which the surface of the several boilers is traversed by the heat, will be to each other, as the products of the ratios of the velocities of the current, or rates of combustion, multiplied into the ratios of the lengths or circuits travelled: thus the

Locomotive is to the Cornish as $6.835 \times 22.142 = 151.34$ to 1.
Ditto Warwick as $5.939 \times 7.237 = 42.98$ to 1.
Ditto Mean wagon as $2.569 \times 10.357 = 26.60$ to 1.

These last found sums truly represent the difference in the periods occupied by the passage of a particle of heat from the grate, till it quits the boilers; they consequently represent the difference in the periods of duration about the boilers of the products of combustion from equal weights of fuel; and we see that the heat of a pound of fuel remains in operation $151\frac{1}{2}$ times in the Cornish, 43 times in the Warwick, and $26\frac{1}{2}$ times in the mean wagon experiments longer than it

remains in the locomotive boiler. In like manner the mutual ratios of the duration of the heat about the Cornish and other boilers respectively are exhibited.

During, however, the periods in which the heat thus acts upon the boilers, it has spread itself over very varying extents and qualities of surface. It issues from the locomotive after travelling only 7 feet; but, in that length, it has passed through 96 tubes *, in as many streams or volumes, and communicated caloric during its transit, to 334½ square feet. In the other boilers, the heat makes its circuit in one unbroken volume, it visits each portion of the boiler in succession, and it finally escapes in the same unbroken volume †. Though, therefore, we have found as above, the ratios of the periods of the duration of the heat in the several boilers, we have yet to find the relative time occupied by it in giving off its caloric to *equal portions* of *surface*. This is the second question to be considered, and is very easily solved.

Proposition 4. The heated areas of the boilers are to one another respectively, as follows: the

Cornish is to the	Locomotive as	961.66 to 334.56, or 2.874 to 1.
Warwick	ditto	151.66 to 334.56, or 0.453 to 1.
Mean wagon	ditto	342.81 to 334.56, or 1.024 to 1.

Proposition 5. The periods of the duration of the heat about equal areas of surface, in the different boilers, will be to each other as the products of the ratios of the velocities of the current, or rates of combustion, multiplied into the ratios of the respective surfaces exposed to absorb the heat: and the

Cornish is to the	Locomotive as	6.835 × 2.874 = 19.64 to 1.
Warwick	ditto	5.939 × 0.453 = 2.69 to 1.
Mean wagon	ditto	2.569 × 1.024 = 2.63 to 1.

Thus we find that the heat from equal weights of coal acts upon equal areas during 1 minute in the locomotive, and during 2.63, 2.69, and 19.64 minutes

* The average number in the boilers experimented upon by M. de Pambour.

† Though in the Cornish, and such of the other boilers as have internal flues, the heat is *split*, during a portion of its progress, into two currents, the latter unite before quitting the boiler, and numberless experiments in setting these boilers have proved the effect to be the same, whether having what is termed a *wheel*, or a *split* draft. I have, therefore, for the sake of simplicity, spoken of the heat making the circuit of these boilers, "*in one unbroken volume.*"

respectively in the other boilers. These then may be properly termed the *relative periods* of *calorific action*, arising out of the structure and practice of each class of boiler.

The truth of these results may be confirmed by another process. It is the same thing to burn, as in the Cornish boiler, one pound of coal in 44.08 seconds, and apply the heat to 961.66 square feet of boiler, as to burn 44.08 lbs. in one second of time, and apply the heat to 44.08 times as great a surface. We should thus find that 42389 square feet would be the area corresponding with that increased rate of combustion for the Cornish boiler. In like manner 2157 square feet would be the equivalent surface for a rate of combustion in the locomotive increased by 6.45 times. The quotient of 42389 ÷ 2157 is 19·64 as above, and represents precisely the difference in time during which the heat elicited from a pound of coal remains in operation under equal surfaces in the Cornish and locomotive boilers respectively.

Were the respective surfaces and structure of boilers of an identical nature, and the practice in each case equally skilful, surface and time would be convertible terms, and ratios obtained by the foregoing process would express with precision the additional areas requisite to be given to one boiler, in order that its evaporative economy should correspond with that of any other boiler compared with it; and though we are ignorant of the heat-transmitting powers of plates of metal of varying thicknesses, as also of the effect of their position in the arrangement of the parts of a boiler; nevertheless, by this method of analysis, we can make ourselves acquainted with the fact, and with the amount of any deficiency in the element *time* by the comparison of inferior with superior boilers; and we are, by these means, informed of the relative degree in which that element is possessed by the different boilers under review.

We know, however, that boilers in every respect alike as to structure, strength, proportion of parts, &c., yield very different results from different treatment, as shewn in numerous instances in Table I.; so that these indications of the effect of *time* must be viewed simply in the light of so many facts attending the comparative practice of the several classes of boiler in the experiments under examination, and not as determinate of any law or rule by which the period of calorific action, or the extent of surface requisite for obtaining equal effects from equal weights of fuel, *in all forms and conditions of boiler*, are governed. There can be no question but that *time*, in some amount, is indispens-

able for the transmission of heat to water through the vessel which contains it —whatever be the structure or strength of that vessel; but it is equally certain that the quality of the fuel, the intensity of the combustion, the nature and strength of the material of the vessel, the form and arrangement of its parts, the proportion of one part to another, and other causes exercise also no inconsiderable influence over evaporative results: I thought it, therefore, better to separate this more analytical, from the more purely practical part of the subject, than to risk confusion by involving the abstract question of time and its influence, in the consideration of the effects produced by other phenomena on the performance of the boilers.

The following Table exhibits, in juxtaposition, the rates of combustion, the evaporative results, the ratios now found of the periods of calorific action, with the gain and loss of effect, by which the influence due to *time*, on the *economy* of heat may be accurately appreciated.

TABLE III.

BOILERS.	Rate of combustion.	Water evaporated by 1 lb. of coal.	Ratios of the total duration of the heat in the boilers from equal weights of coal	Ratios of the duration of equal amounts of heat, about equal areas of the boilers.	Loss of evaporative effect, the Cornish results being unity.	Gain of evaporative effect, the Locomotive results being unity.
	1 lb. in seconds.	212°. lbs.	Locomotive, Unity.	Locomotive, Unity.	Per cent.	Per cent.
Cornish.....	44.08	11.82	155.00	19.64	...	104.85
Warwick.....	38.31	10.32	42.98	2.69	12.69	79.02
Mean Wagon	16.57	8.83	26.60	2.63	24.45	53.03
Locomotive..	6.45	5.57	0.00	0.00	51.18	...

Having thus ascertained the relative periods of the total duration of the heat about the boilers, and about equal areas of the entire surface, which appear so vitally to affect economy, two other important phenomena press themselves on the attention, as having an especial bearing on the *durability* of the boilers; viz., the relative quantity, and the relative intensity of heat given off to those portions of the boilers which are exposed to the direct action, or radiating caloric of the fire. As data for this investigation it is necessary to extract from Table I. the areas of the radiant surfaces, and grates.

TABLE IV.

1 BOILER.	Area exposed to radiant heat.	Area of the Grates.
	Sq. ft.	Sq. ft.
Cornish.....	35.33	23.66
Warwick.....	35.66	23.50
Mean Wagon	39.82	20.87
Locomotive....	41.47	7.00

PROPOSITION 6. The quantities of heat, generated in equal times, are directly as the rates of combustion before found, Prop. 1.

PROPOSITION 7. The areas of the boilers exposed to the radiating caloric are to one another as follows; the

Locomotive to the Cornish as 41.47 to 35.33, or as 1 to 0.851.
Ditto Warwick as 41.47 to 35.66, or as 1 to 0.860.
Ditto Mean Wagon as 41.47 to 39.82, or as 1 to 0.960.

PROPOSITION 8. The quantity of heat supplied to equal areas of these portions of the boilers, will be as the products of the ratios of the rates of combustion multiplied into the ratios of the radiant surfaces; thus the

Locomotive is to the Cornish as $6.835 \times 0.851 = 5.81$ to 1.
Ditto Warwick as $5.939 \times 0.860 = 5.10$ to 1.
Ditto Mean Wagon as $2.569 \times 0.960 = 2.46$ to 1.

These sums indicate that in equal periods the locomotive fire box is supplied with 5.81 times more heat than an equal area of the Cornish, and 5.10 and 2.46 times more heat than the Warwick and mean wagon boilers; and they would express the relative force of calorific action on each square foot, or on equal areas of the surfaces under review, provided the fuel were burnt on grates of equal size; but the intensity of the calorific action—by which the durability of the boilers is principally affected, as also the product in steam from the radiant surfaces—must be measured in a different manner.

The *quantity* of heat supplied to any boiler would not affect the material of which it is composed any the more, whether that quantity or volume were greater or less, provided its *temperature* or intensity remained the same. This

intensity varies greatly in the different boilers. We have seen that on the grate of the locomotive 6.835 times more heat is generated in equal periods than on the Cornish grate; but the grate of the locomotive has only 7 feet area, whilst that of the Cornish has 23.66 feet; and we know that nearly 7 times as much fuel cannot be burned in equal times off a grate less by two thirds than another grate, without the accelerated combustion being accompanied by a very considerable elevation of temperature in the products of combustion. The *intensity* of that combustion must, therefore, be found before its intensity of action on the surfaces exposed to it, can be ascertained.

PROPOSITION 9. The areas of the grates are to one another as follows; the

Locomotive is to the Cornish	as	7 to 23.66, or 1 to 3.380.
Ditto	Warwick	7 to 23.50, or 1 to 3.357.
Ditto	Mean Wagon	7 to 20.87, or 1 to 2.981.

PROPOSITION 10. The relative intensity of the combustion of the fuel will be as the products of the ratios of the areas of the grates multiplied into the ratios of the rates of combustion upon those grates; thus the

Locomotive is to the Cornish	as	$6.835 \times 3.380 = 23.10$ to 1.
Ditto	Warwick	$5.939 \times 3.357 = 19.93$ to 1.
Ditto	Mean Wagon	$2.569 \times 3.727 = 7.65$ to 1.

As a proof that these last ratios are correct exponents of the relative force or intensity of combustion in equal times, they will be found to coincide precisely with the difference in the weights of coal burnt on each square foot of grate per hour, as shewn in Table I., column 20.

PROOFS.

$\overset{\text{lbs.}}{79.3}$ coal burnt on each square of grate per hour, in the locomotive $\div 3.4$ lbs., the rate of the Cornish $= 23.03$ ratio.

$\overset{\text{lbs.}}{79.3} \div \overset{\text{lbs.}}{4}$	the rate of the Warwick $= 19.8$ ratio.
$79.3 \div 10.4$	Mean Wagon $= 7.6$ ratio.

By involving these ratios with those of the respective areas of the radiant

surface, the *intensity of the calorific action* on equal areas, in equal times, will be made manifest.

PROPOSITION 11. The relative intensity of calorific action on the surfaces exposed to radiant heat, will be, as the products of the ratios of the intensity of combustion on the several grates, multiplied into the ratios of the areas of the radiant surfaces; and the

Locomotive is to the Cornish as $23.10 \times 0.851 = 19.65$ to 1.
Ditto Warwick $19.93 \times 0.860 = 16.83$ to 1.
Ditto Mean Wagon $7.65 \times 0.960 = 7.34$ to 1.

The proofs of this solution of the problem will appear by the identity of the last ratios with the quotients of $23.10 \times \frac{35.33}{41.47}$; of $19.93 \times \frac{35.66}{41.47}$; and of $7.65 \times \frac{39.82}{41.47}$; being the respective ratios of fuel burnt, and of radiant surface between the locomotive and the three other boilers.

The results, arising out of the foregoing propositions as affecting the *durability* of the boilers to a great, and their *safety* to a certain extent are classified in

TABLE V.

BOILERS.	Ratios of the intensity of the combustion in the several boilers, to that in the Locomotive, each boiler unity.	Ratios of the intensity of calorific action on the parts exposed to radiant heat to that in the Locomotive, each boiler being unity.
	Ratios to Locomotive.	Ratios to Locomotive.
Cornish.....	1 to 23.10	1 to 19.65
Warwick.....	1 to 19.93	1 to 16.83
Mean Wagon	1 to 7.65	1 to 7.34
Locomotive.....	...	...

I have used the phrase *intensity of calorific action* rather than intensity of heat or temperature, as these sums by no means express the difference in the degrees of the latter. I am not aware that the temperature of boiler fires has ever been ascertained pyrometrically or otherwise, nor do I very clearly see how such indications could be obtained by instruments; nor would any ascertained temperatures denote the effect of what I have termed *calorific action* on the

metal of the boilers. The intensity of that action is not referable to the temperature of the fire alone; it is modified by the distance of the fire from the surfaces exposed to it, in proportion to the squares of those distances; it is modified by the nature of the combustible used; by the nature of the particular gasses which impinge against the metal; by the force with which the radiant particles are thrown off from the grate; also, by the force with which the flames strike the absorbing surface; and I conceive the latter force to be infinitely more influential on the durability of an evaporative vessel, than that of the radiating caloric, as the temperature of flame is much more elevated than that of radiant heat.

Intensity of combustion is the effect of the velocity of the current of air upon the fire; and what I wish to convey by the phrase *intensity of calorific action*, is the joint force arising from the increased temperature, and increased velocity of the product of combustion; and the degrees of durability are the consequences of the effects produced by this force upon the material of the vessels. It is clear that the temperature of the heat in the fire-box of a locomotive is not pyrometrically eighteen times more elevated than that within the furnace of a Cornish boiler; nevertheless, the ratios found in Proposition 11 are facts as regards the relative *action* of the fires, and they are not bad approximate measures of the effects caused by the different systems of practice upon the durability of those portions of the boilers which are exposed to what is called radiant heat, or radiating caloric; a phrase which expresses a very inadequate idea of the severe action to which these surfaces are subjected.

The advantage of slow combustion will, by these investigations, have been rendered more evident than by considering its effect on economy alone; and the influence of *time* as an element concerned not only in the generation, but in the subsequent use of the heat, will be better appreciated.

Having exhibited the relative state in which the heat is found within the furnaces of the several boilers, it would be easy to shew its condition and relative intensity upon the communicative portions; but as a comparison has already been given of its duration about equal areas of the entire surface, I conceive the influence of *time* will be sufficiently understood without pursuing the investigation further.

Table I.

	Place of experiment.	Nature of Coals used.	PARTICULARS OF THE BOILERS. Form of the boilers.	No. of boilers in use.	Length.	Breadth.	Diameter. Outside.	Diameter. Fire-tube.	Mean depth of side, or external flues.	Area of bottom flues.	Area of side, or external flues.	Area of internal flues.	Total area of heated surface.	Surface exposed to radiant heat.	Surface exposed to communicative heat.	Length of circuit made by the heat in one boiler.	Area of the grates.
	Cornwall.	†			ft.	ft.	ft. in.	ft. in.	ft. in.	sq. ft.	sq. ft.	sq. ft.	sq. ft.	sq. ft.	sq. ft.	ft.	sq. ft.
I.	Huel Towan	W	Cyl.	3	36	...	6.0	4.0	...	...	1243	1357	2600	114	2486	152.	72.0
I.	United Mines	W	Cyl.	3	2. 36 1. 40	...	6.4	4.4	...	...	1646	1524	3170	98	3072	158.	70.0
I.*	Mean of Exp. I. & II.....	W	Cyl.	3	36.66	...	6.2	4.2	...	...	1445	1440	2885	106	2779	155.	71.0
II.	East Crinnis	W	Cyl.	3	2. 34 1. 36	... ...	2. 6.9 1. 7.0	2. 4.0 1. 4.5 by 3.5	...	...	1178	1322	2500	57	2443	152.33	37.5
V.	Binner Downs	W	Cyl.	2	30	...	6.0	4.	...	...	686	754	1440	76	1364	132.	48.0
V.	Warwick.....	S.	Wag	3	1. 15.0 2. 12.5	1. 6.0 2. 5.0	...	...	2.6	215.0	240.0	none.	455.0	107.0	338.0	50.66	70.5
VI.	Clithero.....	L.	Wag	1	20.0	5.5	...	...	2.9	110.0	120.0	90.0	320.0	23.62	296.38	91.0	17.6
II.	Ditto	L.	Wag	1	20.0	5.5	...	...	2.9	110.0	120.0	90.0	320.0	35.43	284.56	91.0	20.25
II.	Preston	L.	Wag	1	13.5	8.0	...	...	3.	108.0	108.0	81.0	297.0	46.85	250.15	70.0	27.75
X.	Ditto	L.	Wag	1	13.5	8.0	...	...	3.	108.0	108.0	81.0	297.0	46.85	250.15	70.0	27.75
X.	Ditto	L.	Wag	1	13.5	8.0	...	...	3.	108.0	108.0	81.0	297.0	46.85	250.15	70.0	27.75
II.	Albion Mills	N.	Wag	1	16.0	6.0	...	...	5.6	70.0	236.5	215.0	521.5	35.0	486.5	70.0	24.5
II.	London	N.	Wag	2	15.0	5.5	...	...	2.6	165.0	180.0	none.	345.0	31.5	313.5	56.0	22.66
II.	Ditto	N.	Wag	2	15.0	5.5	...	...	2.6	165.0	180.0	none.	345.0	52.5	292.5	56.0	40.50
III.*	Mean of experiments VI. XIV.	L. N.	Wag	1	36.66	6.5	...	...	3.1$\frac{1}{12}$	118.0	145.06	79.75	342.81	39.82	302.98	72.5	26.09
XIV.	Long Benton	N.	Circ.	2	...	...	1. 12. 1. 13.6	...	4.3	142.0	317.0	none.	459.0	71.0	388.0	52.8	35.10
XV.	Manchester and Liverpool Railway	Coke.	Loc.	1	7.0	...	...	Fire-box. 41.47	...	...	...	Tubes. 293.11	334.56	41.47	293.11	7.0	7.03
XVI.	Killingworth	Coals.	Loc.	1	10.0	...	...	22.56	...	...	...	101.5	124.06	22.56	101.5	...	12.90
	1	2	3	4	5	6	7		8	9	10	11	12	13	14	15	16

† W. indicates Welsh; S. Staffordshire; L. Lancashire; N. Newcastle.

TABLE I.—CONTINUED.

PARTICULARS OF THE COMBUSTION.								PARTICULARS OF THE EVAPORATION FROM THE INITIAL TEMPERATURE OF THE WATER.								
No. of experiment.	Duration of the experiment.	Total weight of fuel burnt.	Weight burnt per hour.	Weight burnt per square foot of grate per hour.	Weight burnt per square foot of heated surface per hour.	Temperature of the water entering the boilers.	Pressure of the steam above the atmosphere.	Total pounds of water evaporated.	Total cubic feet evaporated.	Pounds evaporated per hour.	Cubic feet evaporated per hour.	Pounds evaporated per 1 lb. of fuel.	Cubic feet evaporated by 112 lbs. of fuel.	Pounds of fuel evaporating 1 cubic foot of water.	Pounds of water evaporated by the products of combustion per square foot of grate per hour.	Pounds of water evaporated per square foot of heated surface
	ho. min.	lbs.	lbs.	lbs.	lbs.	o	lbs.	lbs.	cub. ft.	lbs.	cub. ft.	lbs.	cub. ft.	lbs.	lbs.	
I.	24 33	5003	203.78	2.83	0.078	93.8	49.4	52937	847.0	2156.31	34.82	10.58	18.96	5.90	29.94	0.
II.	24	6872	286.33	4.09	0.093	96.0	35.0	72072	1153.15	3003.0	48.04	10.48	18.79	5.95	42.90	0.
II.*	24 16½	5937	245.30	3.46	0.085	94.9	42.2	62504	1000.07	2579.65	41.43	10.53	18.87	5.92	36.42	0.
III.	11 27	3005	262.44	7.00	0.104	...	...	...	...	...	...	...	...	...	...	.
IV.	25 3	5661	222.00	4.62	0.154	...	...	...	...	...	...	...	...	...	...	.
V.	10 30	2961	282.00	4.00	0.019	93.0	2.5	27156	434.5	2586.31	41.38	9.17	16.44	6.81	36.68	5.
VI.	20 25	4144	203.03	11.53	0.634	44.0	5.0	23674	378.78	1160.0	18.55	5.71	10.23	10.94	65.90	3.
VII.	12 25	2016	162.44	8.02	0.507	44.0	5.0	16775	268.40	1351.73	21.62	8.32	14.91	7.51	66.75	4.
VIII.	9 19	2576	276.69	9.97	0.931	76.0	4.0	19312	309.0	2074.3	33.17	7.49	13.43	8.33	74.74	6.
IX.	10	2576	257.60	9.27	0.867	80.0	4.0	21875	350.0	2187.5	35.00	8.49	15.21	7.36	78.82	7.
X.	8	2638	329.75	11.88	1.110	74.0	4.0	22125	354.0	2765.62	44.25	8.38	15.02	7.45	99.65	9.
XI.	10	4030	403.00	16.44	0.772	100.25	2.5	34750	556.0	3475.0	55.60	8.62	15.45	7.24	141.83	6.
XII.	19	5512	290.00	12.79	0.840	101.50	2.5	38625	618.0	2032.9	32.52	7.00	12.55	8.91	89.71	5.
XIII.	22 22	5953	250.13	6.17	0.725	96.5	2.5	48251	772.24	2157.92	34.53	8.10	14.52	7.70	53.28	6.
XIII.*	13 56	3680	271.58	10.75	0.798	77.0	3.68	28173	450.81	2150.62	34.40	7.76	13.91	8.18	83.83	6.
XIV.	1	714	714.00	20.34	1.55	152.0	1.5	5625	90.0	5625.0	90.0	7.87	14.11	7.93	160.25	12.
XV.	1 38	911	557.75	79.33	1.66	60.0	50.0	5664	90.62	3467.75	55.48	6.21	11.14	10.05	493.27	10
XVI.	1	586.88	586.88	45.49	4.73	60.0	...	2620	41.92	2620.0	41.92	4.46	8.00	14.00	203.10	21
	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	

TABLE I.—CONTINUED.

PARTICULARS OF THE EVAPORATION FROM WATER AT THE TEMPERATURE OF 212°.										PARTICULARS OF THE PROPORTIONS OF HEATED SURFACE TO FUEL BURNT, AND WATER EVAPORATED.					
No. of experiment.	Total pounds of water evaporated.	Total cubic feet evaporated.	Pounds evaporated per hour.	Cubic feet evaporated per hour.	Pounds evaporated by 1 lb. of fuel.	Cubic feet evaporated by 112 lbs. of fuel.	Pounds of fuel evaporating 1 cubic foot of water.	Pounds of water evaporated by the products of combustion per square foot of grate per hour.	Pounds of water evaporated per square foot of heated surface per hour.	Square feet of heated surface to 1 square foot of grate.	Square feet of heated surface per lb. of fuel burnt per hour.	Square feet of heated surface per lb. of water evaporated per hour from its initial temperature.	Square feet of heated surface per lb. of water evaporated per hour from 212°.	Square feet of heated surface per cubic foot of water evaporated per hour from its initial temperature.	Square feet of heated surface per cubic foot of water evaporated per hour from 212°.
	lbs.	cub. ft.	lbs.	cub. ft.	lbs.	cub. ft.	lbs.	lbs.	lbs.	sq. ft.	sq. ft.	sq. ft.	sq. ft.	sq. ft.	sq. ft.
I.	59510	952.16	2424.04	38.78	11.89	21.31	5.25	33.66	0.932	36.11	12.75	1.205	1.072	74.67	67.04
II.	80866	1293.85	3369.41	53.91	11.76	21.08	5.31	48.13	1.096	43.88	10.72	1.022	0.911	63.94	56.98
II.*	70188	1123.0	2896.72	46.34	11.82	21.19	5.28	40.89	1.014	39.99	11.73	1.113	0.991	69.30	62.01
III.	...	...	...	...	...	...	...	...	...	66.66	9.52	...	...	...	...
IV.	...	...	...	...	...	...	...	...	...	30.00	6.48	...	...	...	...
V.	30558	488.93	2910.29	46.56	10.32	18.50	6.05	41.28	6.396	6.45	1.61	0.1759	0.1563	10.99	9.77
VI.	27910	446.56	1367.46	21.87	6.73	12.06	9.27	77.69	4.273	18.18	1.57	0.275	0.234	17.25	14.63
VII.	19741	315.85	1590.70	25.45	9.79	17.54	6.38	78.55	4.970	15.80	1.97	0.236	0.201	14.80	12.57
VIII.	22075	353.36	2371.10	39.00	8.56	15.36	7.28	85.44	8.000	10.70	1.07	0.143	0.124	9.00	7.82
IX.	24913	398.60	2491.30	37.95	9.67	17.33	6.46	89.77	8.388	10.70	1.15	0.135	0.119	8.48	7.45
X.	25337	405.39	3167.12	50.67	9.60	17.21	6.50	114.13	10.663	10.70	0.900	0.107	0.093	6.71	5.86
XI.	38846	621.53	3884.60	62.15	9.63	17.27	6.48	158.55	7.448	21.28	1.294	0.150	0.134	9.38	8.39
XII.	43117	689.88	2269.35	36.30	7.82	14.03	8.00	100.14	6.577	15.22	1.189	0.169	0.152	10.60	9.50
XIII.	54116	865.85	2420.21	38.72	9.09	16.29	6.87	59.75	7.015	8.51	1.379	0.159	0.142	10.00	8.91
XIII.*	32006	512.11	2445.23	39.01	8.86	15.88	7.15	95.50	7.166	13.88	1.315	0.171	0.149	10.77	9.39
XIV.	5980	95.68	5980.0	95.68	8.37	15.00	7.46	170.36	13.02	13.08	0.642	0.0816	0.0767	5.10	4.79
XV.	6569	105.10	4030.0	64.48	7.21	12.92	8.66	573.25	12.04	47.59	0.581	0.096	0.080	6.02	5.03
XVI.	3038	48.60	3038.0	48.60	5.17	9.27	12.07	235.50	24.48	9.61	0.211	0.047	0.040	2.95	2.55
	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47

RESULTS of an ANALYSIS of the influence exerted on the Evaporative Economy, and Durability of the Boilers, by the TIME occupied in generating and employing the HEAT.

COMPARATIVE ECONOMY.								DURABILITY.	
No. of experiment.	BOILERS.	Rate of combustion per boiler.	Water evaporated from 212° by 1 lb. of coal.	Ratios of the total duration of the heat in the boilers, from equal weights of coal; the locomotive, unity.	Ratios of the duration of equal amounts of heat, about equal areas of boilers.	Comparative loss of evaporative effect, the Cornish results being unity.	Comparative gain of evaporative effect, the locomotive results being unity.	Ratios of the intensity of combustion in the several boilers to that in the locomotive, each boiler being unity.	Ratios of the intensity of the calorific action on the parts exposed to radiant heat, to that in the locomotive, each boiler being unity.
		1 lb. coal in seconds.	Water. lbs.	Locomotive Unity.	Locomotive Unity.	Loss per cent.	Gain per cent.	Ratios to locomotive.	Ratios to locomotive.
II.*	Cornish	44.08	11.82	155.00	19.64	0.00	104.85	1 to 23·10	1 to 19.65
V.	Warwick	38.31	10.32	42.98	2.69	12.69	79.02	1 to 19.95	1 to 16.83
XIII.*	Mean Wagon	16.57	8.83	26.60	2.63	24.45	53.03	1 to 7·63	1 to 7.34
XV.	Locomotive	6.45	5.57	0.00	0.00	51.18	0·00	0.00	0.00
		48	49	50	51	52	53	54	55

NOTES.

Experiments.

I.—See Mr. Henwood's paper, Transactions of the Institution of C. E., Vol. II. page 56. The additional dimensions of the boilers, not found in Mr. Henwood's paper, were afterwards obtained from him.

II.—The particulars of these boilers, which were carefully measured by the resident engineer, were supplied to me by Mr. John Taylor. The quantities of coal and water are reduced from 8 months to the consumption of 24 hours, from Table II. of my paper. Vol. II. p. 174.

III., IV.—See Mr. Henwood's paper, p. 56.

V.—This is the mean (reduced to $10\frac{1}{2}$ hours, or the period of an actual day's work) of 6 complete months' registration of all the coal consumed, and water evaporated from my own boilers at Warwick, working a 26 horse steam-engine, and supplying a dye-house, wash-house, &c. The coal is the entire weight burnt off the grates, raking at night, and all waste included. The water was gauged from a rectangular reservoir, holding more than a day's consumption, and includes all waste, except that required to refill a boiler when cleaned, and a certain quantity which was blown out of each boiler every night. One of the three boilers was emptied every week, the water being very foul; at the same time the dust was cleared away from the flue bottoms, and the sides of the boilers were well scoured to remove the soot. This latter operation was effectually performed by dragging backwards and forwards a stuffed bag, covered on the outside with coarse woollen cards, and fitting pretty tight between the flue wall and the boiler. By numerous trials it was found that the incrustation of soot impeded the absorption of heat by the boilers (and consequently increased the consumption of fuel) more than the deposit within them, which was considerable, but never suffered to become indurated.

VI.—The sum of Experiments 2 and 3, Table I. of my former paper, page 171.

VII.—See Experiment 5. . . . ditto . . ditto . ditto.

VIII., IX., X.—See Experiments 6, 7, 8 ditto . . ditto.

XI.—Watt and Rennie, ann. 1786. (Farey on the Steam-engine, Vol. I. p. 511.)

XII.—The sum of Experiments 9 and 10, Table I. of my former paper.

XIII.—Ditto . . . 11, 12 and 13, . . ditto.

XIV.—Smeaton's Newcomen engine, an. 1772. (Farey on the Steam-engine, Vol. I. p. 173.)

XV.—The mean of 11 experiments, of which 9 are given in my former paper, Table III., p. 176. (Pambour on Locomotive Engines, pages 175. 320.)

XVI.—See Wood on Railways, 3d ed., pp. 333 and 521.

N.B.—The quantities of water in the columns of evaporation from 212° have been obtained from the real quantities which entered the boilers by the rule and table given in my former paper, pp. 178 and 179.

London, December, 1838. JOSIAH PARKES.

II.—*On Steam-Boilers and Steam-Engines.*

By JOSIAH PARKES, M.Inst.C.E.

PART II.

ON STEAM-ENGINES, PRINCIPALLY WITH REFERENCE TO THEIR CONSUMPTION OF STEAM, AND FUEL.

In the introduction to this branch of my subject, (page 2,) I have already expressed an opinion, that the weight of water, in the shape of steam, which passes through the cylinder of an engine, and produces a given effect, is the best practical measure of the dynamic efficiency of that steam; and that it is the most certain criterion of the economy of different classes of engine in their use of heat.

The habit of comparing the horse-power, duty, tractive effect, or work of an engine—however denominated—with the weight of fuel consumed in doing that work, confounds the results of two dissimilar and unconnected operations; it has mystified the subject; it has greatly tended to obscure our knowledge of the actual degree of excellence attained by the steam-engine; and has retarded the extension of the most effective system of applying steam as a motive force. The generation, and application of steam are distinct problems; they require to be separately treated, and their results to be separately stated. It is the economy of steam which constitutes the dynamic perfection of an engine; it is the economy of heat in supplying steam to an engine, which constitutes the evaporative perfection of a boiler; and it is only by distinguishing the effects of each, that the value of any change of practice, in either department, can be correctly ascertained. These economic properties are totally independent of each other, they may co-exist in the same engine in a maximum amount, or be possessed by it in very different degrees. An acquaintance with the weight of fuel burnt, with the weight of water evaporated, and with the mechanical effect realized, enables us to determine, accurately, the degree of perfection in which these properties are respectively found, in particular classes of engine,

and in particular engines of every class. These are important practical problems, to the solution of which, the attention of engineers and employers of engines should be especially directed. Their solution does not involve the application of extraordinary scientific acquirements, nor does it require experiments of a very nice or difficult nature, nor the use of corrections or data drawn from theoretical sources; any person endowed with common powers of observation and experimental tact, is as capable of discovering the position of an engine, in the scale of economy, as if he were gifted with the genius of a Newton.

So long as engines were constructed chiefly by one maker, their varieties few, and their forms nearly identical, an evaluation of their particular economy, once made, was a nearly sufficient indication of the performance of all; but new forms of engine, new, or extended principles of applying steam have gradually established themselves; and a correct expression, or measure of the performance resulting from these several systems, has become, both for practical purposes and scientific advancement, a matter of deep and acknowledged interest.

It is, therefore, singular that a research into the exact state of the practice, as regards the production and consumption of power, by the different varieties of the steam-engine, should have been so little attended to by engineers.

It has been my humble endeavour to contribute towards the fulfilment of this object, by ascertaining the requisite facts on engines within my reach, and by reducing such other authentic experiments as I could meet with, to the same common standards of comparison. Sufficiently accurate experiments (though not so numerous as might be wished) enable me to investigate several classes of engine, and to determine their relative expenditure of power and heat. They comprise the atmospheric, the stationary non-condensing or high-pressure, the low-pressure condensing, and the Cornish high-pressure, expansive, pumping engines. Table VI. contains the facts established on these four varieties.

Locomotive engines, and their performance, have also been submitted to frequent analysis, but, for reasons which will fully appear in the sequel, I have deemed it requisite to classify them in separate tables; it being first necessary to ascertain the degree of accuracy, to which the various experiments on this class of engine are entitled, before admitting the results obtained from them to a place beside those which deserve implicit faith. It will be seen, too, that the determination of the expenditure of power, for given effects, by other engines, was a necessary step, and will have largely contributed towards the successful examination of the power and performance of the locomotive.

OF THE METHODS EMPLOYED TO DETERMINE THE POWER OF ENGINES.

There are two methods, in common use, for determining the effective power of steam-engines; the one, by ascertaining the resistance overcome; the other, by employing some instrument, such as the *indicator*, to exhibit the load upon the piston; the velocity of the latter being noted at the same time.

The first method is strictly applicable to pumping engines only, or to engines raising appreciable weights; the second presents, perhaps, as near an approximation to the truth, when applied to rotative engines, as that class of engine is susceptible of. It informs us of the amount of force requisite to overcome the friction of the engine, isolated, and of the total force employed under its load. It is customary to designate the difference of these two amounts, the *effective power* of the engine. This definition is not, however, strictly correct, as the additional friction, to which the moving parts of an engine are subjected by the load, is thus involved in the expression, and reckoned as a part of useful effect; by that amount, therefore, inconsiderable though it may be, the determination of effective power, by this method, is in excess.

On the other hand, the power, deduced from the duty of a pumping engine, falls short of the real effective power of the steam used. This engine, when applied in the best manner, does not act directly on the water; the force of the steam is employed to lift the pump rods, the weight of which, together with their friction, that of the accessory balance beams, or *bobs*, &c., and of the water in the pipes, constitute the true load upon the engine; the duty, therefore, ascertained by the column of water displaced, does not represent the entire effective power of the steam. The value of these surplus frictions above the proper friction of the engine, unloaded, should be added to the duty, for the purpose of bringing the Cornish, or other pumping engine, to an exact comparison with a rotative one, the effective power of which has been ascertained by the indicator.

It is thus evident that *useful effect* is not always synonymous with, nor a true measure of *effective power*. The duty is the true useful effect of a Cornish engine. The method of ascertaining it is the fairest possible, for practical purposes, and it will be apparent that the errors in estimating effective power are against the Cornish, and in favour of the rotative engines. I have thought it advisable to point out the mutual bearing of these errors in computing effective power, in order to remove all suspicion, that the Cornish engine may

owe any portion of its greatly superior performance to an exaggerated estimate of the duty from which its effective power is deduced.

The indicator may be usefully applied to a Cornish engine, to ascertain its *absolute* power; but as that engine cannot be isolated from its load, so as to determine the separate value of its own friction, the indications of the instrument would not serve to place the estimation of its effective power, on an equality with that obtained by them from a rotative engine. The indicator, however, by ascertaining the total pressure on the pistons of both engines, would inform us of the absolute and relative power derived from the expansive, and unexpansive systems of using steam, by enabling us to compare the loads deduced from the mean pressure on the pistons, with the weights of water as steam, which have respectively passed through the cylinders, and overcome those loads. But, in order to obtain correct results, it is necessary to apply the instrument to the vacuum, as well as to the steam side of the piston of a single engine, it being well known that the amount of vacuum in the cylinder is not identical with that in the condenser, nor instantaneously produced; it being also well known that the rapidity with which the vacuum is formed, affects the power of an engine in no inconsiderable degree. It was the perception of the influence of these phenomena on power, and economy, which led Watt to invent and apply the indicator, as an accurate means of ascertaining the variable and incrementary amounts, both of pressure and exhaustion, which take place *within* the cylinder, during a stroke of the piston, rather than confide in deductions from uncertain data acquired from *without* the cylinder.

There is a third method, by which the absolute power of steam, communicated to an engine, can be deduced; but, as its value depends on a perfect accordance between the results of experimental and practical science,—an accordance yet unascertained,—and since many precautions are requisite to secure true results from this test, it has been seldom resorted to by practical men.

The volume of water converted into steam, being known, and also the volume described by the motion of the piston within the cylinder, the load upon the piston is deducible from a knowledge of the elastic force of steam, corresponding with the ratio which the volumes of water and steam, consumed by the engine, bear to each other. It being assumed that the densities, and consequent pressures of steam are correctly assigned to these relative volumes, it is necessary, for practical use, to detect and fix the amounts of those portions of the steam, which are ineffective as power. These quantities vary in different

engines, and must be ascertained for each engine. A portion of the steam is condensed by contact with surfaces of lower temperature than itself; another portion is useless, from its filling the spaces contained between the piston, the cover and bottom of the cylinder, and steam-valves. The proportion of these ineffective to the effective quantities must, consequently, be deducted from the volume of water, before its true ratio to the volume of effective steam can be found; and care must be taken that no loss of water, or steam, take place, in other ways, during the experiment.

There is another source of inaccuracy, which, if undetected, would altogether vitiate this test; it frequently exists to a considerable extent, unsuspected; or, if suspected, its amount eludes determination. I refer to *priming*. The power of an engine, whose boilers are subject to prime, cannot be investigated with success by the method of the volume of steam, as will hereafter appear in the examination of the locomotive engine. No value can be attached to this method, unless the steam be chemically pure; unless it be free from particles of unvaporized water, mechanically suspended in it, and accompanying it into the cylinder. But in all cases where the steam is pure, this test may be advantageously used (under the foregoing precautions) to compare the absolute power of an engine, as exhibited by the indicator, or by known resistances, with the absolute power resulting from the whole force of the steam which has operated upon the piston *.

OF THE MEASURES OF EFFECT.

The performance of pumping engines is commonly defined by the weight in pounds which has been raised one foot; and this quantity, being divided by the number of fixed measures, or weights of fuel, which have been expended in raising it, gives a product termed the *duty;* a phrase which very significantly expresses its meaning. The relative performance of different engines is thus conveniently found by a comparison of the duty obtained from equal measures, or weights of fuel.

The performance of rotative engines is commonly referred to the weight in pounds which has been raised one foot, during a minute of time; which sum,

* It is very desirable that a series of experiments should be instituted to demonstrate, if possible, the degree of accordance which may subsist between results obtained by this process on the large scale of practice, from the same engine working under greatly different pressures, and the determinations of chemical philosophers.

divided by 33,000 pounds, gives the expression of effect in terms of *horses-power;* a conventional phrase, which, however inconsistent it may be in its application to many of the uses of the steam-engine, and however vague it may be as a measure of the power of a horse, is, nevertheless, as good as any other expression we could adopt, for the purpose of bringing the economy of various engines to a common standard of comparison.

It is usual to compare the relative performance of engines, of the rotative class, by the number of pounds of fuel consumed by each horse-power in an hour. The weight of fuel thus ascertained, has, therefore, raised 1,980,000 pounds one foot in 60 minutes, and it is the duty done by that weight of fuel; whence, by simple proportion, the duty is obtained in terms of any other weight of fuel.

In like manner, by noting the time in which the duty of the pumping engine has been accomplished, it is readily reduced to terms of horses-power; so that the effects derived from engines, of both classes, are comparable under both denominations.

I have, accordingly, exhibited (Table VI., columns 8, 9, 10) the amount of duty of each engine as performed by 1 lb., 94 lbs. (or the Cornish bushel), and 112 lbs. of coal respectively, for reasons which will appear in the sequel. The weight of coal consumed per horse-power per hour is also shewn in column 15.

We have thus two expressions of the effect obtained by equal weights of coal from the engines under comparison; but those expressions convey no information as to the real expenditure of power in the production of a given effect.

OF THE EXPENDITURE OF POWER.

The ponderable element of steam is water; its consumption by an engine is appreciable; and it is now assumed, almost universally, that the sum of its imponderable element, heat, is a constant quantity, in steam of all specific gravities. The elastic force of steam is also generally assumed to be proportional to its density; thus, equal amounts of heat and water are expended in the generation of equal power, at whatever pressure steam be used by an engine. The correctness of these assumptions, founded on the experiments of many distinguished philosophers, will receive additional confirmation from some experiments of my own, purely practical, to be related hereafter *. Admitting, then,

* Vide page 70.

these properties of steam to be inseparable from its composition, the expenditure of both power and heat, is truly measured in all engines, by the weight of water consumed, as steam; and the effects produced, being known, and reduced to a common denomination, the weights of water so consumed indicate the positive, and relative efficiency of the steam in different engines.

Admitting, however, the supposition that the elementary constituents of steam may not yet be absolutely defined, with equal accuracy, throughout the whole range of its scale; and that the proportions of heat to water, at different pressures, may undergo changes not yet correctly ascertained, the weight of water as steam, expended in the production of a given effect, remains unimpaired in its value as a test of dynamic efficiency. It becomes, indeed, for these very reasons, the more valuable for practical uses, than a test derived from the consumption of any given measure of steam.

By knowing the evaporation from the boilers, and, consequently, the weight of water as steam which passes through an engine, we grasp the principal fact of practical consequence to the engineer; a fact which is free from all uncertainty in its nature; which involves no dispute as to its qualities or characteristics; which is, in no respect, mixed up with the chemistry of steam; which is independent of all theory, and requires no correction to determine its value. So long, too, as the steam is taken immediately from above the surface of the water in the boiler; and so long as it enters the cylinder of an engine unchanged, it is in that state which every one understands by the term steam, in its common acceptation; and the weight of water, which has passed from the boiler in that state, and produced a certain effect, appeals conclusively to the understanding as indicative, in a comparison of engines, of their respective economy in the expenditure of power.

I have, accordingly, computed the weight of water as steam equivalent to the production of a horse-power in each engine, and, also, the duty effected by one pound of steam. These sums (columns 11 and 16) denote the positive and relative *efficiency of the steam* in the different engines; and, in like manner, the positive and relative *efficiency of the fuel* is exhibited (columns 10 and 15).

Under the head *Comparative Economical Results* (columns 21 and 22), I have reduced these respective indices of efficiency to terms of the ratios which each of them bears to the results obtained on Watt's engine at the Albion Mills, which latter I have assumed as unity. The table therefore exhibits,

	Columns.
1. The precise value of each engine in its use of steam . .	11 and 16
2. The precise value of each engine in its use of coal . . .	10 and 15
3. The precise value of the boiling apparatus attached to each engine, as a generator of steam	17
4. The comparative efficiency of the steam, or economy of power, in each engine	21
5. The comparative efficiency of the fuel, or economy of the combustible, in each engine	22

OF THE PROPORTION OF BOILERS TO ENGINES.

Having thus arrived at an exact knowledge of the weight of water as steam, consumed by each engine in the production of equal effects; or, in other words, of the power resulting from the expenditure of equal weights of water as steam, I am able to connect the boilers with the engines, and to shew the relative extents of heated surface which have been employed to furnish their power; and it will presently be seen how insufficient equal measures of surface would be, to supply equal power, with equal economy, to different classes of engine.

The effective horse-power of each engine, at the time of the experiment, is shewn in column 12; and the heated surface of boiler, per horse-power, in column 18, obtained from the total area belonging to the same engines, given in the table of boilers (column 12, page 45). It would appear, by this comparison, that the Cornish engineers employ about three times more surface than Watt did, reckoned on the horse-power of engine; but, column 21 informs us that the horse-power is produced in a Cornish engine, (taking the mean of experiments VI. and VII.,) by somewhat less than two-fifths of the steam consumed at the Albion Mills: thus, the Cornish engine, for equal nominal power, has nearly eight times as large a boiler surface as that allowed by Watt's practice. But this measure exhibits no true comparison of the proportion of generative area to the power expended by different engines; for, the functions of the former are irrespective of the engine, and have reference only to the fuel burnt, and to the water evaporated. By column 15 it is seen that the Cornish engine (using the mean before stated) consumed only $2\frac{1}{2}$ pounds, whilst Watt's engine

consumed $8\frac{1}{2}$ pounds of coal, per horse-power per hour. Dividing the heated areas of the boilers by these respective amounts, a product is obtained which shews the positive and relative extents of surface for each pound of coal burnt to supply the two engines, with equal power, in equal times. These results are set down in column 19, and the true relation of the boilers is found to be as 19 to 1, instead of 3 to 1.

Column 20 contains the heated area of the boilers, proportionate to the weights of water as steam, which have produced equal power, in equal times, in the two engines, which gives a ratio of nearly 15 to 1 to the Cornish, in excess over Watt's practice, instead of 8 to 1.

It will now be apparent why I could not assign the relation which subsists between the boiling apparatus of any particular engine as regards surface, and the power of that engine, until its consumption of steam and fuel were ascertained. It will also be seen that, if the Cornish engineers had followed a rule of boiler-surface, based on the mere nominal horse-power of engine, instead of wisely continuing to augment the proportion of boiler, whilst they were gradually reducing the expenditure of steam as power, they would have lost 3-10ths of the superior economy of their system.

These last measures, contained in columns 19 and 20, are, however, no fixed rules as regards the Cornish practice; they apply only to the particular instance cited; for, the proportionate boiler-surface necessarily varies with the degree of economy of steam and fuel. Experiments VIII. and IX., on two other Cornish engines, exhibit a large diminution in the consumption of fuel, and water as steam, for equal power, compared with experiments VI. and VII.; and, though I have not the particulars of the boilers, we may be well assured that their evaporation was fully equal to that from the others *; and, consequently, that their heated areas were in the same proportion. Assuming this to be the case, the surfaces of the latter, reduced to the terms of columns 19 and 20, would be larger; and, for experiment X., we should find them increased from the ratios of 19 and 15, as in experiments VI. and VII., to 27 and 21 to 1, in excess over Watt's. The performance of the expansive pumping engine is subject to fluctuations; and the measure of boilers, taken as above, necessarily changes with the power consumed; the one being a constant, and the other a variable quantity.

Well made rotative unexpansive engines, whether of the condensing or non-condensing kind, can vary but little in their respective consumption of steam,

* Vide page 63.

for a given power, at different periods; but their boilers, to produce steam with equal economy, must necessarily be proportioned to the weights of water, which have to be vaporized, in order to create the power. Thus, a high-pressure non-condensing engine would require $\frac{4}{7}$ths more generative area than the condensing engine, to be upon a par with it in calorific economy, as the former consumes $\frac{4}{7}$ths more steam than the latter, for equal effects.

All engines may be furnished with boilers possessing equal evaporative economy, that being a result derived from the amount of surface, and other qualities previously explained; and the best rule, if it be necessary to prescribe one so obvious, is to adopt that measure of surface which has afforded the greatest evaporative effect from a given weight of fuel. The weight of water as steam, consumed per horse-power per hour, being shewn in column 16, the total vaporization required in an hour, by any good engine of the classes examined, is obviously obtained by multiplying that weight by the number of horses-power of the engine in question; and the total generative area requisite for realizing the maximum effect hitherto obtained from coal, will be to employ as many superficial feet of heated surface, as pounds of water used per hour. This is the Cornish measure, as will appear by consulting the table of boilers, and is referable, strictly speaking, to the Cornish form, strength, and setting of boilers, rate of combustion of fuel, &c.

OBSERVATIONS ON THE EXPERIMENTS, AND THEIR RESULTS.

The first experiment in the table is by Smeaton, on his improved Newcomen engine.

This experiment is particularly interesting, as a record of the consumption of power, by a good specimen of the earliest steam-engine, which can be considered as having exerted any important influence on the economy of the arts. It was also the great predecessor and type of Watt's engine; whose improvements upon it consisted chiefly of additions affecting its economical use of steam, retaining the great principle of condensation, together with the mechanical structure, viz. cylinder, piston, beam, and self-acting valves; apparatus which continue in use at the present day, with the advantage only of superior workmanship, and of more perfect mechanical contrivances.

Newcomen's engine must ever be regarded by engineers with a species of veneration, as the great forefather, and founder of the whole race of steam-engines. The numerous, beautiful, and highly scientific combinations to which

the steam-engine owes its modern perfection, have always appeared to me to be entitled but to secondary claims in the award of merit, compared with that due to Newcomen and his compeers, who, in reality, created it. Prolific as the genius of Watt must be regarded; immeasurably important to mankind as were the consequences of his discoveries; we must not overlook the fact that the steam-engine was already invented; that it was already a mighty instrument of mechanical force; and, though Watt greatly reduced its expenditure of power by one contrivance, and, by another, made it applicable to rotative purposes—thereby enlarging the bounds of its usefulness beyond calculation—we must bear in mind that the great problem was already solved; and that, strictly speaking, Watt's improvements were only corollaries, requiring, doubtless, more than ordinary talent to apply them, yet they were improvements, not creations.

Following, in these remarks, the order of invention, rather than the order of the experiments in the table, it appears that the economy of Watt's rotative condensing engine, at the Albion Mills, (Experiment V.,) with reference to its consumption both of steam and fuel, was double that of Smeaton's Newcomen. The performance of that rotative engine, measured by the duty, corresponds, also, so nearly with the best work done by Watt's pumping engines in Cornwall*, that the comparison holds good for both kinds.

No improvement in the science of the unexpansive condensing engine has taken place since Watt's day; and there are, probably, very few engines of this class, which perform their work with so small a waste of steam, or fuel, as the one experimented upon at the Albion Mills, which approached very nearly to perfection in the use of power, obtainable from that principle.

The next great advance, in the economy of power and fuel, had its origin in Cornwall; the merit of it is wholly due to the Cornish engineers; to whom belongs, also, the merit, not only of having carried the system adopted by them for pumping engines, to a degree of perfection which eclipses the performance of every other description of engine, but of applying it to rotative purposes, with effects far exceeding those of the common condensing engine. The rise and progress of this vast improvement is so well traced by Messrs. Lean, in the useful and highly instructive compendium already referred to, that all observations from me would be a work of supererogation.

Experiments VI. and VII. exhibit, I believe for the first time, the precise

* Lean's Historical Statement of the Duty of Steam Engines in Cornwall, page 8; compiled for the British Association for the Advancement of Science, 1839.

amount of the superiority of this engine over others, and it is seen that, when doing an 80 million duty, it excels Watt's by $2\frac{1}{2}$, and Newcomen's engine 5 times, in economy of power; and, that it excels them $3\frac{1}{2}$ and 7 times respectively, in economy of fuel. It is also seen that a Cornish engine (Experiment IX.) has exceeded the same engines $4\frac{1}{2}$ and 9 times respectively, in economy of power; and $5\frac{3}{4}$ and $11\frac{3}{4}$ times respectively, in economy of fuel.

Such has been the progress of invention, estimated by economy in the expenditure of power, between the years 1772 and 1835.

It is not my intention, were it even in my power, to enter here into any analysis of the causes of the greatly superior effect derived from steam, when used on the Cornish system. My principal object has been to arrive at practical determinations of the consumption of power, considered apart from the consumption of fuel, by each engine. The doubts entertained by many persons of the accuracy of the Cornish statements of duty, and, consequently, of the effects derived from the use of expansive steam, are chiefly attributable to the Cornish method of rating the duty by the fuel, rather than by the power really expended. By separating these results, I hope that I shall have removed one great cause of distrust, which has certainly operated to retard the extension of the expansive system of using steam to several other classes of engine, to which it is well suited; and that I shall have cleared away obstacles to our arriving, at some not distant period, at a successful discrimination of the inherent value of the expansive action of steam, and of its value when combined with the mechanical action, and peculiar work of a steam-engine. These are problems, of a nature distinct from the practical facts now sought, and require very nice experiments for their solution. It has been customary, on the one hand, to assign the superiority of the Cornish engines solely to the use of steam of high elastic force greatly expanded; and, on the other hand, to dispute the value of expansion. Too much, perhaps, has been claimed for that principle, and too little granted, by the controversialists. It is now shewn (column 21) in what ratio over other engines, the efficiency of steam is increased, by the Cornish system of employing it, separate from all consideration of the greater or less work done by the coal, whether arising from peculiarities in the structure of the boilers, from the calorific strength of the coal, or other causes. We are not, however, justified in concluding that the great dynamic advantage, still resulting, is due to the single circumstance of that engine being worked by highly elastic steam, greatly expanded; other causes may contribute to produce the effect.

It is necessary, also, to guard against conclusions which might be deduced, from a comparison of the effects of the Cornish engines in the table, with the pressures on the piston and degrees of expansion, set down in columns 5 and 6. The pressures given were not ascertained by any instrument, (excepting at Huel Towan,) and must be considered only as estimations, not as facts. The pressure upon the piston during the interval which occurs between the first admission of steam into the cylinder, and the instant of shutting it off, may be very variable; that it was so, in several engines to which Mr. Henwood applied the indicator, is evident from the diagrams he has given, annexed to his paper. (Trans. Inst. C. E. Vol. II.) At the Huel Towan engine, when the steam in the boilers was at a pressure of 47.1 lbs. above the atmosphere, it varied from 12.3 lbs. to 7.3 lbs. per square inch on the piston, during its admission into the cylinder; which latter was its elastic force, at the instant of closing the steam-valve *. I adduce these facts, with the view of shewing the impossibility of determining the precise amount of pressure on the piston, from the degree of *wire-drawing* the steam; and as a caution against expectations of deducing any valid theory of the action of the steam, in these Cornish engines, from the particulars of pressure and expansion, contained in the Table, which are only approximations to the truth.

A few observations are necessary, with reference to the correctness of the weights of water as steam, assigned as the consumption of the Cornish engines. The Huel Towan engine was experimented upon by Mr. Henwood, with all due care, during 24 hours; the results of the United Mines experiments are drawn from a registration of water, fuel, and duty, during eight months of continuous work. In my paper on evaporation, (Trans. Inst. C. E. Vol. II. p. 175,) I noticed the very considerable differences in the monthly returns of water vaporized at the latter engine; which, however, I considered to be accounted for, by the statement given in the note to that page. The subsequent investigation of the boilers of these two engines, the near correspondence between their extents of surface exposed to heat, their near equality in the rate of combustion, &c., as shewn in the Table of boilers, justify the assumption, that the United Mines experiment is worthy of belief. As such I have introduced it, being in close agreement with Mr. Henwood's results; and I can see no more reason for doubting the one than the other. Mr. Hosking, the resident engineer at the United Mines, obligingly remeasured the boilers, and supplied me with all the particulars relative to them and to the engine, which I required to complete the Tables.

* These facts were communicated to me by Mr. Henwood.

I have applied the mean of evaporation obtained by a pound of coal from the boilers of the above named engines, as data for the determination of the consumption of water as steam, in two others; viz., the one at Holmbush, (Experiment VIII.), whose performance and fuel were so accurately ascertained by Mr. Wicksteed; and of the Fowey Consols engine, (Experiment IX.,) which formed the subject of the celebrated trial in 1835.

Knowing, as we now do, the product of steam from a given surface of the Cornish boilers; knowing also, that the Cornish engineers habitually allow that, or a nearly corresponding, extent of surface, for the production of similar effects; we are as much justified in assuming a similar performance to result, in all cases, as we know would take place, if an engine of the same dimensions as Watt's, at the Albion Mills, with equal power of boilers, were experimented upon.

Of the amount of duty at Holmbush, there cannot exist a doubt; the water raised was weighed, and measured. Mr. Wicksteed's experiment thereby establishes the general accuracy of the Cornish statements of the duty done by their engines; and, as the duty of the Fowey Consols was the subject of a most patient and careful trial, during 24½ hours, by a committee of perfectly competent persons, I regard it—though exceeding in amount all previous and subsequent trials—as authentic as any other on record.

As the evaporation was not ascertained at either of those engines, a question might be started, whether their additional duty, compared with the United Mines and Huel Towan engines, may not have arisen from a superior evaporation per unit of coal, rather than from a superior dynamic effect, obtained from the steam generated. In the case of the Fowey Consols, I have satisfied myself, by enquiries of Mr. West, the maker of the engine, that the increased performance was due to the better use of the steam in the engine, not to any greater evaporative product from those particular boilers. Mr. West states that during the trial in 1835, the evaporation was 100 gallons from the temperature of the hot-well, by the bushel of 94 lbs., that is 10.63 lbs. of water vaporized by 1 lb. of coal. In reply to a second, and more recent enquiry, Mr. West writes, as follows:

"I have made another trial, for the purpose of ascertaining the quantity of water evaporated, by the consumption of a bushel of coals, (94 lbs.,) and find it come very near my former experiment. The trial was as follows. The feed plunger-pole and its stroke were accurately measured; the number of strokes made in 24 hours amounted to 1920; the diameter of the pole is 6 inches; the

stroke 2 feet 6 inches, being equal to a little more than 3 gallons per stroke, or 5873 gallons in the 24 hours, in which time 60 bushels of coals were consumed, making 98 gallons to a bushel. The temperature of the water in the hot-well was 85°, and on entering the boilers from the warming tube, about 180°. The pump and boilers, at the time I made the experiment, were in good order. The full length of the stroke of the feed-pump is 2 feet 7 inches, from which I have deducted one inch, to allow for waste, &c.

"When our engine at Fowey Consols was on trial, the steam, on entering the cylinder was about 27 lbs. pressure on each square inch, and cut off at $\frac{1}{4}$ stroke."

This second experiment gives 10.41 lbs. as the evaporation by 1 lb. of coals; and the mean of the two is 10.52 lbs. The mean of the two experiments at the Huel Towan and United Mines, is 10.51 lbs. The evaporative results are, therefore, identical *, and the difference between the duty of the engines, is attributable, only, to the difference in their respective expenditure of power, to produce equal effect; or, in other words, to their respective dynamic efficiency.

Experiment IV., made by myself at Warwick, on my own engine of Boulton and Watt's construction, is the result of observations continued for several months, and is strongly corroborative of the results obtained at the Albion Mills.

In the conditions of the two experiments, there are, however, some points of importance to notice. It is requisite to distinguish between the nature of the loads on the two engines; the Albion Mills engine, grinding corn; the Warwick engine, driving worsted spinning machinery. The load of the first was, consequently, by far the most uniform of the two. My experiment includes the waste, and loss of steam, attendant on occasional short stoppages; to which all manufactories are liable. The engine was fully loaded, but, like all manufacturing engines, its load was subject to continual, almost to momentary, fluctuations. It had, generally, to be started, with such a load upon it, three times a day, as to require *blowing through* each time—an operation which swallows much steam—but no estimate of that useless consumption could be formed.

The fuel, at the Albion Mills, was that expended, simply, in supplying 10 hours steam to the engine. The fuel, at Warwick, worked the engine during $11\frac{1}{2}$ hours each day, and includes the loss to maintain the boilers hot, during

* The correspondence is the more satisfactory, as the experiments were made at different engines, at different periods, by different persons, and by different methods.

$12\frac{1}{2}$ idle hours, out of the 24 hours; but, as no steam was ever permitted to blow away through the safety valves, the consumption of water as steam, which did the work, is absolutely correct.

At the period of my entering on these experiments, I was unacquainted with the indicator; but that instrument was, in the course of them, applied to the engine by Mr. Creighton, of Soho; and its load, with all the machinery at work, was determined by him to be 26 horses power. The mean load was, probably, somewhat less than this, but it is impossible to say by how much; nor can a nearer approach to accuracy be obtained from any engine, working under similar circumstances. It is a specimen of the *practical* expenditure of power and fuel, in a manufacturing engine; the nature of which does not, however, admit of sufficiently exact results to justify *theoretical* deductions. The pressure of steam on the piston, sometimes exceeded, and sometimes fell short of the pressure of the atmosphere; and I have called it equal in column 6. Deducting $\frac{1}{10}$th for the loss of steam at each stroke, arising from the steam passages, jacket, and other waste, the ratio of the volume of steam consumed, to that of the water which produced it, comes out as 1720 to 1; shewing the force of steam upon the piston to have about balanced the atmosphere.

The daily and hourly registration of the water as steam which passed through the cylinder, gave me an insight into the exact state of the engine, and its load, at all times. The practical value of this knowledge can scarcely be overrated. The engine was an old one; the piston packed with hemp; the cylinder a good deal worn; but we were warned of any increase in the expenditure of steam, by the hourly consumption of water; so that a defect was searched for, and a remedy applied with the least possible loss of steam. Though the mean consumption was 70 lbs. of water per horse power, per hour, I have known 80 lbs. consumed when the packing was worn, or the vacuum less perfect than usual; and I was never able to reduce the consumption of water lower than 65 lbs. per horse, per hour, for many hours successively.

I once made an experiment to ascertain, during a whole day, the consumption of steam by the engine, working idle—and, afterwards, with the whole machinery, working idle, i. e. unclothed with wool—with the view of arriving at a knowledge of the real power consumed by the processes of the manufactory; but, unfortunately, I either did not note down the results, or have lost them. Such experiments would be very valuable to manufacturers; for, though the indicator informs us of the pressure on the piston, it does not disclose the expenditure of power; the registration of the water, alone, unfolds this know-

ledge. It checks, and tests the industry and skill of the attendant engineer; it informs him of the state of his boilers, of his engine, or of that of its load, which, from carelessness, is often greatly increased for want of oil, &c.; and it instructs the master as to the numerous causes of waste, and the expensive consequences of negligence.

In comparing the results of this engine with those of the Albion Mills, it would appear, from the duty tested by the fuel, that the former had the advantage; whence, it would be concluded, that the Warwick was the better engine of the two, but when brought to the true test of the *efficiency of the steam* in each, it is seen that the Albion Mills engine had a slight preponderance in its favour. The evaporative superiority was obtained in the Warwick, through the careful nursing and system of management explained in my paper on Evaporation, and at pp. 12 and 13 on the Boilers.

Experiments II. and III., on two high-pressure non-condensing engines, were, also, made by myself. I attached great importance to both these experiments, and conducted them with all the care of which I was capable, being unacquainted with any practical determinations, by engineers, of the consumption of steam by this class of engine.

Experiment II. The engine drove a silk mill, and being employed by its owner to reset the boilers, I determined, at the same time that I ascertained the amount of evaporation, previous to altering the boilers, to ascertain, also, the load on the engine. I used the indicator, and continued the experiments during three successive days; the mean of these is set down in the Table. The water was injected into the boilers from a measured vessel;—the valves, and piston were perfectly tight, but the cylinder was not jacketed; and as the valves were only single, (set half way between the top and bottom of the cylinder,) I attribute the excess in the consumption of water as steam by this engine over the next related experiment, to the greater quantity of ineffective steam wasted in the passages, and to condensation in the cylinder, for want of a jacket.

Experiment III. This engine drove bark mills, and other machines. I was engaged to arbitrate a dispute between the maker, and owner of the engine, respecting its power. I found the engine perfectly well made; the cylinder jacketed, and clothed outside; the valves double, with no extraneous spaces; the piston had metallic packings; but the boilers were insufficient in surface, and ill set. Evaporative economy was, consequently, low. I had no

indicator, and used the mercurial column, with a stop cock, to determine the pressure on the piston. The Table gives the mean of numerous trials, which varied but little from each other. There was, originally, a loss of power in this engine, arising from too contracted eduction pipes; these were enlarged, so as to relieve the piston from any counter resistance beyond that of the atmosphere; and the results stated, were obtained after this alteration.

In both experiments, I ascertained that the boilers did not prime, (of which I had some suspicion from their small dimensions,) by testing the evaporation, with the man-hole covers removed, which corresponded very nearly with the product from equal weights of coal, burnt at the same rate as when the engines were at work.

The comparison of the duty done by 1 lb. of coal, and 1 lb. of water, as steam, (columns 10 and 11,) shews that the respective efficiency of these two engines, would be incorrectly estimated by the fuel consumed. The expenditure of power, for equal effects, by each, is seen truly in columns 11 and 16. The near coincidence between the results of these two trials is satisfactory; and, I think, conclusive as to the expenditure of power by this class of engine, though it is somewhat less in amount than I have commonly heard assigned to it. I am acquainted with a mine engine, which was worked either on the condensing, or non-condensing principle, according to the supply of water; and the coal accounts shewed that, in the latter state, when using steam at about 15 lbs. pressure, it consumed about twice as much fuel as when condensing. I have heard similar statements from several respectable quarters, and think it very probable that such would be the approximate results, from engines so converted; but experiments of this nature are, evidently, inadequate to determine the true relation of the efficiency of steam on the two systems.

The Table thus comprises determinations of the positive, and comparative excellence of the three great classes into which steam engines may be divided. Of these there are varieties, which must partake, in a greater or lesser degree, of the advantages of each system.

The Cornish engineers have recently extended the application of their system to rotative purposes, by engrafting rotation on single, as well as double-acting engines; and they have already attained the high duty of 60 millions and upwards *—a convincing proof that the benefit to be derived from the use of greatly expanded steam, is not limited to pumping engines. Marine en-

* Lean's Monthly Reports.

gineers are slowly following in the wake of the Cornish; prudentially, too, as there are many other considerations of importance besides economy of power, in a steam-vessel; but there can be no doubt that diminished expenditure of power, and, consequently, of fuel, will accompany the closer imitation of the Cornish practice. The combination of two engines, and the momentum of a steam-vessel, are elements, *per se*, particularly favourable to the application of this system in its fullest extent, to the purposes of marine locomotion; but the extent, to which it can be practically adopted, is a question to be settled only by experience, carefully, and gradually acquired.

It is a question, also, whether the extreme use of the Cornish system is suitable for those manufacturing engines, one great and essential quality of which is uniformity of motion. A steady velocity in the motive power, is of such consequence in cotton spinning, and several other of the arts, that any loss of it would be dearly bought by economic gain. The momentum of machinery of this description, is but trifling; and an equivalent must be found for it, in order to obtain the whole value of the Cornish expansive system. With the degree of uniformity of motion, attained by expansive rotative engines in Cornwall, I am unacquainted; this should be a matter of investigation by persons desirous of availing themselves of their economy, for uses which require such precise, and unvarying speed.

The continuous working of a steam-engine is productive of a large saving in fuel. A residence of some years in Manchester enabled me to inform myself of the average consumption of coal, by the best engines in that town and neighbourhood; which, being rated at about 1½ hundred weight, per horse-power, per diem, or 15 lbs. per horse, per hour, may be considered as below, rather than above the mean. Of this quantity, it is estimated that one third is wasted in raising the steam each morning—in the loss occasioned by meal-time stoppages—and in the consumption of the night. Much of this waste is, doubtless, owing to careless management, to the neglect of covering boilers, &c.; such is, however, the expenditure of fuel, in present practice, in Lancashire. A Cornish engine, when doing an 80 million duty—a performance lower than Cornish engineers would guarantee—accomplishes the same work as a Manchester engine, with one sixth the expenditure of fuel. It has been suggested, that fully three-fourths of the entire effect of a Cornish engine would be realized, and the most uniform of motions obtained, by employing this engine to raise water upon a wheel, and thus transfer its power to machinery. The engine might then be

kept continually at work, and the horse-power would be obtained by the consumption of about 3 pounds of coal per hour.

This would, probably, be the cheapest of all means of obtaining power, for stationary purposes on railways, where fixed engines must lose, by waste, a very much larger proportion of steam and fuel than manufacturing engines, owing to the greater irregularity of the demand upon them. The pumping engine may be kept at work without intermission; its rate may be accurately accommodated to immediate, or anticipated wants; and the use of a reservoir, of very moderate dimensions, would, at all times, secure a supply of power, available at a moment's warning, by night or day. These arrangements are practicable in all situations where a sufficiency of water, for injection, exists; little waste would arise from the wheel, its water being continually returned to it, and that waste could be made good from the water discharged by the air-pump.

OF THE STANDARD MEASURE OF DUTY.

The present system of rating the duty of steam-engines in Cornwall, by the heaped imperial bushel measure, of coal, is open to great objections.

If it be necessary, from long usage, to purchase coals by measure, they should be weighed to the engine; as the calorific value of coal does not depend on bulk, but on the purity, and quantity of its carbon. This objection to measure, has, to a certain extent, been appreciated, and it has been found requisite to fix the *duty weight* of a bushel. It appears to me that as the weight of a bushel of coals is so various, and as it has been found needful to reduce these varying bushels to some common weight—94 pounds being the present Cornish standard of comparison in the Monthly Reports of duty—it would be better to adopt some multiple of a ton for that purpose; or, still better, to estimate the duty, by the effect of a single pound of coal. The difference in weight, of the bushel of coals, from mines in the same district, and different districts, is greater, perhaps, than is commonly imagined. From the published Minutes of Proceedings of the sessions of the Institution*, it appears that the bushel of coals has been ascertained by several engineers, to vary from 80 to 112 lbs.; and Mr. Henwood found, at three engines, the respective weights of the bushel of coals in use, to be 100, $92\frac{6}{10}$, and $88\frac{3}{10}$ lbs. (Trans. Inst. C. E. Vol. II.)

Fuel, unaccompanied by a statement of its evaporative product at each en-

* See Minutes Inst. C. E., Session 1838, pp. 3 and 7.

gine is, at best, but an imperfect standard of the merit of engines. A knowledge of its consumption is a necessary, and important item in accounts of expense, as it determines the money cost of working an engine; and we have seen the success which has attended the unwearied and skilful efforts to diminish it; but a more accurate standard of dynamic efficiency is requisite. It is possible that coal may not always continue to be the sole combustible employed to generate the power of engines; it may be exchanged for anthracite, coke, peat—substances already in partial use—or steam may be generated by artificial fuels, or by processes now unknown, and unimagined. The work done by any given weight of water as steam, is a sure index of the quality of a steam-engine; it is a measure unaffected by variable calorific agents; and, so long as the engine continues to be worked by steam, so long will the performance of different engines be accurately gauged by their respective expenditure of water as steam.

I will venture to suggest the use of the *pound of water as steam*, as the most convenient and correct standard of duty which can be adopted. In Table VI., column 11, this standard is applied to all the engines examined. Cornish engineers will perceive the ease with which the tyranny of custom would be shaken off, and the facility with which old associations would be melted into a new one, by the adoption of this standard. It happens—from the circumstance that the Cornish evaporation, in the best conditioned engines, amounts to 10½ pounds, for each pound of coal burnt; and, from the circumstance, that the adopted weight of a bushel is but little short of 100 pounds—that the duty done by a pound of steam is nearly the one thousandth part of that done by a bushel of coal. The transition, therefore, would be easy and natural from millions, raised by a bushel of coal, to thousands of pounds, raised by one pound of water as steam. The image of effect would, also, be much more clearly impressed upon the mind by reference to this measure of force, rather than to the force obtained from a bushel of coals. Every one can understand and appreciate the weight of a pound; as well as the degree of force he would have to exert in raising that pound a foot in height; and, being informed that a pound of water, converted into steam, is capable—through the medium of the engine—of elevating one hundred and twenty-six thousand times its own weight, to an equal height, the mind would receive, not only a distinct image of the vast force of steam, and of the account to which it is turned by the ingenuity of man,

but it would imbibe an accurate idea of the true measure of the power of the steam-engine.

Great additional value would be conferred on the monthly reports of the Cornish engines, by incorporating in them two such columns as 10 and 11 in the table. The evaporation from the boilers of each engine might be stated separately, or it might be omitted; as the weight of water vaporized by a pound of coal, in each case, would be found by dividing the duty obtained from the pound of coal, by that obtained from the pound of water as steam.

In column 8, I have exhibited the duty equivalent to the use of one hundred weight of coal; but, though a multiple of the ton, I think the hundred weight would be objectionable as a standard, from its raising, still higher than the bushel, the expression of the load overcome. The millions of pounds, vanquished by the bushel, have already sufficiently tried the ingenuity of the expounders of the steam-engine, by inducing them to convert weight of duty into bulk; or to involve in it, as they have sometimes done, strange conceits of time and space, in order to represent, as they imagine, a less complex notion of this simple proposition.

OF THE CONSTITUENT HEAT OF STEAM.

The weight of water, as steam, would not be a true measure of the relative consumption of heat by different engines—which is the economical result sought—if a given weight of water, as steam, at different elasticities, contained variable amounts of caloric. Experiments, to determine the constituent heat of steam, conducted with great care, by various methods, and leaving little to be wished for, have been made by several philosophers. Their conclusion, that the sum of heat in steam, at all specific gravities, is a constant quantity, is generally received as a law of nature.

It occurred to me that it would be practicable to perform a set of experiments, within certain limits, which would have the effect of confirming, or disproving this assumed fact, by ascertaining the quantity of water which a given weight of coal would convert into steam, at various pressures. It was requisite that these experiments should be made on the same boiler, with the same kind of coal, and that an equal weight of coal should be consumed, during each experiment, as nearly as possible in the same time; or that equal quanti-

ties of water should be evaporated, under the different pressures, as nearly as possible in equal times.

The following Table gives the result of 28 experiments, made during 28 successive days, on vaporization from the boiling point, to 60 pounds pressure per square inch above the atmosphere. They are presented, as essentially confirming the doctrine first made known by Watt and Southern; as practically conclusive that there is no material difference in the consumption of fuel, to convert equal weights of water into steam, of the elasticities cited; and, consequently, that the relative efficiency of steam in engines is solely due to the manner of using it, not to any change in its chemical constitution at different pressures.

TABLE VII.

Number of experiments.	Pressure above the atmosphere.	Temperature of the steam.	Total weight of coal burnt.	Mean weight of coal burnt, per experiment.	Evaporation in cubic feet.	Mean duration of experiment.	
No.	lbs. per sq. in.	Temp.	lbs.	lbs.	Cub. feet.	Ho.	min.
4	0	212°	800	200	20	10	
1	5	226.30	199	199	20	9	55
1	10	237.64	202	202	20	10	1
3	15	247.94	585	195	20	9	50
2	20	256.78	396	198	20	10	2
1	25	264.82	204	204	20	10	4
1	30	272.02	200	200	20	10	
1	35	278.80	203	203	20	9	58
2	40	285.04	202	202	20	9	59
2	45	290.76	408	204	20	10	5
3	50	295.96	615	205	20	10	
3	55	300.76	624	208	20	9	57
4	60	305.06	840	210	20	10	2

I will now proceed to relate the manner of conducting these experiments, as their value entirely depends on the precautions taken to ensure accurate results.

The boiler employed, was, in its exterior form, precisely similar to an ordinary locomotive one; but of different dimensions and internal structure. The fire-box, inside, was 4 feet square, and the grate, consequently, 16 square feet in area. The heat then passed through five tubes, 10 inches diameter inside, and 5½ feet in length, contracted at the smoke box end to 5 inches diameter, in the last 12 inches of their length. The chimney was twenty feet high from the ground, and 15 inches diameter, with a damper in it near the summit. In the first

place, I ascertained how much water the boiler would hold, in the visible length of the glass gauge, so as not to leave the top of the fire-box uncovered, nor yet to rise so high as to endanger the accuracy of the experiments by priming. The water was both weighed and measured into the boiler, by imperial standards; a scale was adjusted to the glass gauge, which sensibly denoted a cubic foot, and admitted of subdivisions; and I found I had 20 cubic feet of water to operate upon. The capacity, thus ascertained and graduated, served for all the experiments. This being done, I ascertained the weight of coal, which would, probably, be required to evaporate the 20 cubic feet of water; but, as I wished to render the combustion very perfect, and ensure, as far as possible, that no uncombined air should pass through the fire, I diminished the area of the grate one half by bricking it over, air-tight, and left the air spaces, between the bars, $\frac{1}{16}$ths of an inch wide, the bars being $\frac{3}{4}$ of an inch in breadth.

As the experiments in contemplation would last many days, and it being essential that the coals should be as nearly uniform in purity and strength as possible, I went to a neighbouring pit to procure them; and into the pit, that I might assure myself of every lump coming from the same vein, and the same part of it. I thus procured 3 tons, and had every lump broken into pieces which would pass through an inch and a half ring; rejecting every piece which exhibited the slightest symptom of impurity. The coal was then carefully covered up from rain or dampness.

It may not be uninteresting here to state, that I tested the coal as to the moisture it contained, when raised from the pit, and found it to be about $\frac{1}{20}$th of its weight. As a measure of precaution, also, I each day weighed 1000 grains of the coal submitted to combustion, dried it thoroughly in a warm oven, and ascertained its loss, which varied from 4 to 6 per cent., during the 28 days of the experiments; so that no change took place in the humid contents of the coal.

A thermometric steam-gauge, graduated for temperatures and pressures, (made by Mr. Adie, optician, Liverpool,) was applied to the boiler, without which I should have been unable to have kept the fire, and rate of evaporation, so steady as I desired. The safety-valve was similar to those commonly used on locomotive-engine boilers, and had been carefully weighed, when the degrees of pressure were marked on its spring balance. During these experiments, the spring balance was released from its hook, and weights were suspended to it, that it might rise freely at the blowing point of each experiment. The scale of the

valve was graduated from 10 lbs. to 65 lbs. above the atmosphere. It rose very evenly, throughout the whole range, in correspondence with the pressures marked on the thermometric gauge, excepting that it was uniformly somewhat later in its indications than the former. This is explained by Mr. Adie, (with whom I communicated on the subject,) by his stating that the temperatures, and corresponding pressures, marked on his instrument, were from 2 to 4 degrees of Fahrenheit below those determined by the French academicians, as they were not corrected for the portion of the mercury exposed to the cooling of the atmosphere—a difference which was to be expected. The correspondence between this thermometer (with the correction supplied) and the safety-valve, were thus a proof—though a rude one—of the accuracy of the determinations of the French philosophers. Mr. Adie's graduations were from experiments, made by himself, with steam from the boiler of the locomotive engine, Venus, brought to a mercurial column, on the Liverpool and Manchester Railway.

These arrangements being made, a few preliminary experiments shewed that about 200 lbs. of coal would be required, and that about 10 hours would be the time occupied in vaporizing the 20 cubic feet of water, at the slow rate of combustion, which I thought best fitted to give correct results. As the damper did not fit quite close enough, to restrain the fire sufficiently, I diminished the aperture of the five tubes, at the point of exit into the smoke-box, by one half.

The experiments were prosecuted as follows.—Each morning 200 lbs. of coal were weighed by myself, the boiler having been previously pumped somewhat fuller than the level of commencement, and the steam raised to the pressure of the intended experiment. When the thermometer shewed signs of falling below that pressure, and the fire was unable to elevate it, the water was drawn off to the zero or upper division of the glass gauge; 185 lbs. of coal were immediately put lightly and equally over the whole surface of the grate, and the fire door instantly plastered round with clay. As the draught and combustion were very slow, it required about half an hour before evaporation commenced, and the thermometer usually fell 2 or 3 degrees during the first quarter of an hour; until the coals became a little ignited. The fire door was opened at the sixth hour to examine and level the fuel; and as the evaporation, which was noted every quarter of an hour, was nearly alike each day and each hour, it became easy to judge how much more coal would be required; and my practice was to add 10 lbs. at that hour; clay up the fire door again; and, as soon as the steam began to fall, to add as much more coal

as would complete the evaporation of the 20 cubic feet. The duration of each experiment is reckoned from the moment of the rising of the valve, at the respective pressures, till it closed from the exhaustion of the fire.

At the termination of each day's experiment, the boiler was well cleansed by blowing off, through several cocks, when the steam was yet up, but the fire extinguished. The fire-box, grate, and flues were swept free from all deposit or attached soot, and the boiler replenished with water for the following day's experiment. I attended each day at the starting of the experiment, and until the regular rate of evaporation exhibited itself. I visited the boiler two or three times during the first six hours, and was always present from the sixth hour till the conclusion. When any thing occurred to cause me to doubt the accuracy of an experiment, it was repeated on the ensuing day; and though, in reality, every one of the experiments is entitled to as much confidence as another, it will be seen that as I approached the higher pressures, I made more experiments on each.

The columns, mean weight of coal, and time, express the means of the number of the experiments on the same pressure; the variation in the weight of coals burnt, did not, however, exceed 4 lbs., nor the time more than five minutes, in any of these, so that I have thought it unnecessary to encumber the table by inserting other than the mean time.

I am confident that not an ounce of water was wasted any one day. I wished to have experimented at 65 lbs., but one of the rivets wept a little at that pressure, and I abandoned it.

It will be remarked that, from the commencement to the termination of each day's experiment, my assistant had nothing to do but to observe his thermometer, note the rate of evaporation, regulate his damper, and occasionally hand the safety-valve to maintain the pressure of steam uniform. The thermometer indicated, with great delicacy, when the damper required opening or shutting, by a single notch. No water had to be pumped; the fire had not to be stirred; nor could any thing occur to derange the observations.

I, therefore, consider these experiments as substantively conclusive, for all practical purposes, that equal quantities of heat enter into the composition of equal quantities of water in the shape of steam, at all the temperatures and pressures noted; and, consequently, that the sum of the sensible and latent heat, in steam of the specific gravities belonging to these pressures, is a constant quantity, as previously determined by nicer experiments. The greatest difference in the results

of any two experiments is 5½ per cent., and I feel no hesitation in expressing my own conviction, that the small increase in the consumption of coal observable at the more elevated pressures, was simply owing to the circumstance, that, in consequence of the increased temperature of the absorbing water, at those pressures, the heat necessarily quitted the boiler at a somewhat higher temperature than at the lower elasticities.

I omitted to state that, in the first two of the four experiments at the boiling point, the man-hole cover and safety-valve were removed, and two cocks opened, that the steam should have the freest possible exit, in order to confirm my faith in no unvaporized water passing over with the steam in the subsequent experiments. In the two others, stated as from 212°, the whole of the steam escaped through the safety valve aperture, exhibiting an elevation of about two degrees of temperature. At 5 lbs. on the inch, the safety valve was replaced, and regulated by hand, as its own weight, with the lever, spring-balance, and rod, equalled 10 lbs.

It may be said, and with truth, that though equal volumes of water were evaporated, the weights were not identical, owing to the dilatation of the water for its temperature; but this would cause so slight a difference, that I preferred the plan of evaporating equal bulks, previously heated in the boiler to the experimental degree, to that of pumping the supply during each experiment: for, in the 28 days, the water I was using would have varied nearly 15 degrees in temperature, on entering the boiler.

The experiments were made in January and February, 1837. The boiler was much exposed, being fixed upon a machine for agricultural purposes, then on a bog; but the whole being covered with a roof, no rain or snow entered, and the boiler was clothed with sacking and tarpaulins, so that the varying temperature of the atmosphere, out of doors, could have but little effect upon it. Every precaution was used to preserve, as nearly as possible, an uniformity of temperature about the boiler.

The coal was extracted from one of the pits belonging to Wm. Hulton, Esq., of Hulton, near Bolton, a coal well known in Lancashire for the purity and strength of its coke; and I took the opportunity of making four experiments on the latter combustible; viz. one at the boiling point, one at 20 lbs., one at 40 lbs., and one at 60 lbs. pressure. The consumption, respectively, was 148, 149, 152, and 153 pounds to evaporate the 20 cubic feet of water, in 10 hours; being about 25 per cent. less than the coal from which it was prepared. It must, however,

be observed, that, as the coke required more draught than the coal, I was obliged to widen the air spaces between the bars, and to open the ends of the five tubes of the boiler, so that a greater waste took place, of droppings into the ash-pit, and of heat through the chimney.

TO FACE PAGE 76.

No. of experiments.		Duty in lbs. of coal raised one foot high, or of steam, power per minute.	Evaporation in lbs. of water by 1 lb. of coal from the initial temperature of the water.	Heated surface of boiler per horse power.	Heated surface of boiler for each lb. of coal burnt by one horse power per hour.	Heated surface of boiler for each lb. of water as steam used by one horse power per hour.	Comparative economical results. Efficiency of the steam; Albion Mills Engine, Unity.	Comparative economical results. Efficiency of the fuel; Albion Mills Engine, Unity.	Date of experiment, and authority.
		Water as steam, lbs.	lbs.	sq. ft.	sq. ft.	sq. ft.	Ratios.	Ratios.	
		A							
I.	Long B	138.88	7.87	11.33	26.03	3.305	0.501	0.489	Smeaton, 1772. (Farey on the Steam Engine, Vol. I.)
	Non-								
II.	Conglet	125.00	7.00	.....	.....	.....	0.556	0.482	J. Parkes, 1823.
III.	Paris	120.00	6.00	.....	.....	.....	0.579	0.437	J. Parkes, 1827.
	Cc								
IV.	Warwic	70.00	9.17	17.50	59.63	6.500	0.992	1.127	J. Parkes, 1820.
V.	Albion 1	69.50	8.03	10.43	60.28	7.500	1.000	1.000	Watt, 1786. (Farey on the Steam Engine, Vol. I.)
	Cornis								
VI.	United 1	28.65	10.44	30.26	1156.93	114.112	2.425	3.137	Lean's Monthly Reports, 1837. The duty assigned to this engine is the average for the same months as that of the evaporation from the boilers noted in my paper, Trans. Inst. C. E., Vol. II., p. 174. J. P.
VII.	Huel To	24.21	10.58	29.19	1144.03	107.390	2.868	3.765	Henwood, 1831. (Trans. Inst. C. E., Vol. II. p. 58.)
VIII.	Holmbu	16.61	.....	.....	.....	.....	4.180	5.454	Wicksteed, 1836. (Trans. Inst. C. E., Vol. II. p. 63.)
IX.	Fowey C	15.68	.....	.....	.....	.....	4.435	5.786	Mining Journal, Oct. 31, 1835, and published report of the trial; also Lean's Report on the Cornish Engines, compiled for the British Association for the Advancement of Science. 1839, p. 97.
		16	17	18	19	20	21	22	23

OF THE LOCOMOTIVE ENGINE.

The locomotive, as an instrument of mechanical power, differs only by its use of the blast from the fixed non-condensing engine; it is the same engine set on wheels; it receives, and transfers the force of the steam, in a manner identically similar to a stationary engine; and its effects are ascertainable and measurable by processes, common, and applicable to each. In its locomotive character, it resembles the marine steam-engine; both engines form integral parts of the mass of matter set in motion by them; the power of each is communicated to the mass, by contrivances varying only in their adaptation to the media on which they operate; but, both engines are at rest, relatively to the mass moved, and to the media of motion. The power and effects of both engines are, therefore, recognizable, and appreciable by one or other of those methods of investigation previously described, and employed in practice, as they may best suit the special circumstances, work, and nature of different engines.

The method resorted to by experimenters on the locomotive, has been that of attempting to determine the amount of resistance opposed to its progress, in preference to that of ascertaining the power expended in overcoming the resistance. The solution of either of the two problems would be equivalent to the solution of both, it being an axiom in mechanics, that power and resistance are constantly in *equilibrio;* the amount of one of these forces, being ascertained would, consequently, be a true measure of the amount of the other.

The search after the sum of the resistance, by the process of analysing its several constituent quantities, has been attended with perplexing difficulties; as that sum arises out of forces, which, though distinct, are yet so numerous and variable, that the determination of the exact quantity assignable to each, at any one velocity, has hitherto eluded the sagacity and industry of all the experimenters who have explored the subject. That the investigation has failed of success, will, I think, be demonstrated by the following examination of such results and conclusions as the several labourers in this inviting and fertile field of experiment have presented to us, as facts; or which, as deductions from natural laws, ought, in their opinion, to command universal assent, and be received as a creed based on immutable principles. I think it will be apparent, from the indications of the tests to which I propose to submit these so-called facts, and deductions, that the first are but erroneous estimations, and the second, unwarranted, and untenable hypotheses.

The exposure of fallacies is far from being an agreeable task; but, if truth —fabled as being concealed at the bottom of a well—is to reward our researches, we must not be scared from its pursuit, by the weight of superincumbent matter to be removed, before the light shines on the object sought. I have laboured diligently, though humbly and less publicly than several other enquirers on this subject; I have carefully studied the exposition of their experiments and deductions; and, in discussing them, I trust that no difference of opinion with the authors, whose works it is necessary for me to examine, will be construed into any want of respect on my part for men whose talents, and persevering exertions entitle them to the esteem of all who seek to illustrate the practice, or principles of engineering science.

The first analyst, in the order of time, who applied himself to the solution of the several problems connected with the locomotive engine, was M. de Pambour. His work* appeared in 1836. I received it when engaged in nearly daily practice and experiment, with an engine precisely similar to the railway locomotive, though applied to a very different species of locomotion. The consumption of steam and fuel, by this engine, had necessarily occupied much of my attention, it being requisite to inform myself of the cost of power, in order to determine the important item of expense, for the purposes to which the engine was destined. I had experimented upon the additional resistance occasioned by the blast, at certain pressures; upon the expenditure of water as steam, at different loads, and at different velocities; and, from a previous knowledge of the consumption of steam, for given effects, by fixed high-pressure, and other engines, I felt to have in my hands a safe test, within certain limits, of the accuracy of my results. These will appear in their place in the sequel.

M. de Pambour's experiments arrested my earnest attention, and I immediately set to work to reduce them to the standards, by which the engines are compared in Table VI. In this there was no difficulty, as the tractive force was given; whence both the duty, and horses-power were deducible.

The results obtained from the very first investigation surprised me, it appearing that, if the estimate of resistance were correct, a duty had been accomplished, and a horse-power had been created, by a much smaller expenditure of water as steam, than was possible even by the condensing engine. I imagined, therefore, that some error had crept into my calculations; or, that some extraordinary economic principle belonged to the locomotive engine, which had

* A Practical Treatise on Locomotive Engines upon Railways.

escaped my notice; or, I was forced to conclude that the resistance assigned, viz. 8 lbs. per ton, was considerably greater than could have been overcome by the steam, at the velocity of the engine under examination. I repeated the calculations, repeated the experiments on the engine under my charge, and found the same discrepancy between M. de Pambour's and my own results. These calculations were made upon the Atlas, at the velocity of 9.72 miles per hour, being the first among the experiments given in the following Tables.

A more attentive study of this author's work, disclosed to me, also, that he had not ascertained any data by which to estimate the surplus resistance to the effective steam in a locomotive, over a fixed high-pressure engine, occasioned by the counter elasticity in the blast-pipe; the amount of which would reduce still lower, the consumption of water as steam, for a given effect. In addition, I found that the tractive force was considered by the author to be uniform at all velocities; inasmuch as he assumed the resistance to the progressive motion of the train, to be equal at all velocities. This was a puzzle; for, it seemed self-evident, since an engine could not drag so heavy a load at a high, as at a lower velocity—and, since it appeared, from the evaporative data, that a larger consumption of water as steam took place at the higher than at the lower velocities, in every instance—it seemed, I thought, self-evident that, rated on the ton of matter moved, the tractive force must have been, from some cause or other, increased at the higher velocities, in a proportion not greatly differing from the inverse ratio of the loads. Some great misconception appeared to me to pervade the whole of these experiments, for, if the deductions were erroneous in one case, they were so in all, being all based on data similarly obtained; but, whether the error arose from fallacious deductions, or inaccurate observations, it was out of my power to clear up the mystery. Strong doubts, too, arose in my mind as to the correctness of the evaporative data, my experience of locomotive engines having assured me that priming, in a greater or less degree, existed in all of them; and, by that amount, whatever it was, the water consumed as steam would have to be again diminished.

After making several abortive attempts to reconcile the results of these experiments with those on my own, and other engines, I determined to defer a more rigid scrutiny of them, to a future period, in the hope that M. de Pambour himself, or other experimenters, would throw additional light on the subject; that they would supply the necessary corrections; or renew the investigation on some fresh basis.

The examination, which my little leisure then permitted, led me to reflect on the fitness and accuracy of the method adopted by M. de Pambour of deducing the resistance to traction, from the laws of gravitation; and I was disposed to conclude that no certain results could come out of this method; that no practical data, worthy of confidence, as to the amount of resistance on a level, could be derived from observations on engines and trains moving down inclined planes; and that, if positive and appreciable weights could not be employed to determine the tractive force at various velocities, recourse must be had to some other measure of effect. No definite experiments had, at this time, been made with the intent to separate the resistance of the air, from the frictions of motion; nor to resolve several other items into which resistance and friction might be subdivided, and which enter, as distinct quantities, into the composition of the sum of the resistance. Further, I felt disposed to doubt the practicability of performing any set of experiments which could satisfactorily evaluate the ever fluctuating, and inconstant quantities of the various frictions, and resistances, to which a locomotive engine is subject, from the peculiar character of railway traffic.

These opinions stimulated me to endeavour to discover some certain criterion of the mechanical effect produced by a locomotive engine, at all velocities; some criterion which would apply as practically, as appropriately, and as distinctively to locomotive, as *duty* to pumping, and *horse-power* to rotative engines; and, thereby, enable us to draw a rigorous comparison between the increments of resistance, and the increments of velocity. If the work of a locomotive furnished a measure of this nature, it seemed to me that several problems of great practical consequence might be solved; and I considered it of far less importance to be able to distinguish (were it possible) the precise value of each particular unit of resistance, than to determine, correctly, the relative sum of resistance, and the relative expenditure of power, at all velocities, and under all circumstances.

This study opened to me a glimpse of a method of comparing the performance of railway engines, which will be treated of in a separate section.—To perfect, and prove its application and powers, additional experiments to those of M. de Pambour were requisite; and it was necessary to await the gradual development of the railway system, by the opening of new lines, and the publication of experiments on locomotives which the extended use of them would, doubtless, occasion. A sufficient number of these has accumulated, and I shall

first proceed to examine their results, according to the opinions and deductions of their authors, in the order of the dates of their publicity. They consist of the labours of M. de Pambour, Mr. Robert Stephenson, Mr. Nicholas Wood, and Dr. Lardner.

Experiments by M. de Pambour. M. de Pambour's celebrity as a railway analyst—the adoption of his views and conclusions by a large class of persons—the later rejection of them, or substitution of still higher values of resistance by another class, equally confident in the sufficiency of their experiments, and in the truthfulness of their deductions—demand that his experiments should undergo a very minute and rigid investigation. It is the more fitting, too, that I should examine these experiments with the utmost care, as my own conclusions (so opposed as I have already explained them to be to those of the author) are based upon the comparison of the data he has supplied, with the ascertained, indisputable facts regarding the effects of other engines exhibited in Table VI. For this purpose it has been necessary to reduce the whole of his data to terms which permit the particular and comparative values of the power and effect to be shewn, when tested by similar measures. Tables VIII. IX. X., in which these results are classified, contain also those obtained from the data of other experimenters.

Table VIII. exhibits the particulars of the engines, and all the quantities required for ascertaining the duty accomplished at the resistance assigned to the load in each case, which is separated into the gross and useful duty obtained by the consumption of a pound of water as steam, and a pound of coke, respectively. A few words only of explanation are requisite on this head.

The work done by a locomotive engine is clearly a *duty*, in the strict sense of the term. Whether an engine be considered as dragging a load whose resistance is 8 lbs. or any other number of pounds, per ton; or, whether a weight of 8 lbs. for each ton of matter moved, descending over a pulley and attached to the load, be considered as the motive force, the result is the same; and that resistance—which is the real load on the engine—being ascertained, the whole duty, or effect, is found by multiplying this sum by the distance travelled in feet; and the product divided by the number of pounds weight of water and coke consumed, respectively, gives the expression in those terms. In estimating duty, the friction of the engine is disregarded; its amount formed a subject of distinct investigation by M. de Pambour, and is not included in his estimate of the resistance at 8 lbs. per ton; nor does it enter into the duty calculated for the other engines in the Table. The locomotive, and its tender are

here considered only as carriages, and their resistance forms part of the gross duty, as the steam overcomes it all; the useful duty is deduced from the tractive force required by the useful load, or weight of wagons and carriages, without the engine and tender.—Thus, supposing the following facts to be accurately ascertained; viz.,

1. The tractive force per ton of matter in motion;
2. The space passed over in feet;
3. The consumption of water as steam, during the trip;
4. The consumption of coke . . . . . ditto;

we have all the elements given to find the duty, gross, or useful, performed by the steam, and coke. One example of the computation will suffice for all; vide Atlas, Exp. I.

Resistance, or tractive force, 1655.20 lbs. × 155,760 feet (in 29½ miles) = 257,813,952 lbs. raised one foot; which, ÷ 8260 lbs. water consumed as steam = 31,212 pounds raised one foot, by one pound of water as steam.

Table IX. contains the requisite data for computing the pressure against the pistons deduced from the sum of the resistances, first calculated on the assumed resistance overcome at the velocity of the wheel, in each experiment; and, also, the pressure on the pistons deduced from the ratio of the volumes of steam and water consumed. So far as this table relates to M. de Pambour's experiments on the resistance overcome, the pressure on the pistons of the several engines is calculated from the amount of the special frictions and resistances assigned by him. He observes, in the work referred to, page 239, "*that all the power applied is to be traced in the effect produced, and there is not a single pound of which the use may not be pointed out.*" I have already stated that the author assumed the resistance to progressive motion to be a constant quantity at all velocities; and that an important item of resistance to the effective power of the steam, had not been analysed by him, viz. the pressure against the piston, above the atmosphere, from the blast.

The pressure deduced from the sum of the resistances, given in column 29, for M. de Pambour's experiments I. to X., is composed, 1st, of the friction of the engine without load, which includes the resistance opposed to it as a carriage, in common with the train; 2dly, of the additional friction brought upon the engine by the load; 3dly, of the resistance of the load, at 8 lbs. per ton. According to the author, these three items include all the resistance overcome by the steam, excepting that occasioned by the blast, in excess above the pressure of the atmosphere.

The amount of the latter should, therefore, be ascertainable by comparing the whole force exerted by the steam on the piston, with the force assigned as requisite to overcome the aforesaid three, out of the four component parts of the total resistance. The difference between these pressures should represent the precise amount of the counter elasticity of the steam in the blast-pipe; and, thus, if the data were correct, "all the power would be traced in the effect produced, and the use of every single pound of steam pressure on the piston would be accounted for." To find the value of this latter force in each experiment, it is, therefore, only necessary to discover the total pressure which has acted on the pistons, and to deduct from it the pressure required to balance the ascertained resistances; and it is evident that their difference would be the unknown quantity—or blast pressure.

To arrive at this result, I have calculated, for each case, the volumes of water, and steam, consumed in equal times; and I have used M. de Pambour's own table for finding the elasticities corresponding with the respective ratios of the volume of steam to that of the water*. The total pressure of the steam upon the pistons; the pressure due to the ascertained resistances; and the resulting difference, are accordingly shewn in columns 29, 30, and 31; which, with the known pressure in the boiler, will be found useful tests of the accuracy, or inaccuracy, of the author's data. The application of the same tests to the other experiments, will be attended with equally searching effects, as to their exactness. (*See Table at the end of this Paper.*)

In order that no question may arise as to the fidelity of the sums set down in column 29, I have brought out from M. de Pambour's work, and given at foot, an example of the computation, using his own data and formulæ†.

In explanation of the sums assigned to the volumes of water, and steam

* The Theory of the Steam Engine by Le Comte de Pambour, page 76. 1839.

		Tons.	lbs.	
† Atlas. Exp. I.	Weight of engine	11.4	152	friction without load.
	„ tender	5.5		
	„ load	190.0		
		195.5 × 8 (lbs.)	1564	resistance of train.
			195.5	additional friction of engine owing to the load.
Sum of resistance, at the velocity of the working wheel			1911.5	

1911·5 lbs. × 5·887 ratio of the velocity of the wheel and of the piston = 11,253 lbs. re-

consumed, it is necessary to state that the whole quantity of water evaporated (as given in column 11) is reduced, first by one-fourth, in conformity with M. de Pambour's own instructions, (pages 182 to 184,) as he found that, "of all the steam generated, a quarter was lost through the safety valves"; and the remainder is diminished by $\frac{1}{20}$th, being the proportion of ineffective steam, or the quantity wasted, at each stroke, by filling the passages between the valves and cylinders.

The volume of effective steam, therefore, (column 26,) being found by the multiplication of the capacity of the two cylinders, into the number of double strokes of the piston per minute, (columns 24 and 25,) and divided by the volume of water, corrected as above, (column 27,) the ratio which they bear to each other is ascertained (column 28); and the pressures corresponding with these ratios are obtained from M. de Pambour's Table above cited. One example will serve to illustrate the process of computation; Atlas, Expt. I.

sistance produced on the piston; which, $\div$ 226 area of the two pistons in inches = 49·77 lbs. resistance on each square inch of the surface of the pistons.

The following is the computation of the total resistance, and pressure on the pistons, for each case, from the respective weights of engine, tender, and load, according to the above rules. The friction of each engine was determined by the author separately, and I have used the invidual amount, so found, in preference to the mean of all, as the difference between them is considerable.

Experiment.	Total resistance. lbs.		Per sq. inch. lbs.	Friction of engine. lbs.
II.	1305.26 ×	$\frac{5.887}{226}$ =	34	 152
III.	517.85 ×	$\frac{5.887}{226}$ =	13.49	 152
IV.	429.20 ×	$\frac{5.887}{226}$ =	11.17	 152
V.	485.35 ×	$\frac{5.887}{194{\cdot}40}$ =	14.68	 187
VI.	487.63 ×	$\frac{5.887}{190}$ =	15.10	 136
VII.	903.06 ×	$\frac{5.887}{190}$ =	27.97	 108
VIII.	441.09 ×	$\frac{5.887}{190}$ =	13.66	 108
IX.	548.20 ×	$\frac{5.887}{190}$ =	16.97	 109
X.	614.34 ×	$\frac{5.887}{190}$ =	19.03	 109

(*Treatise on Locomotives*, pages 134 to 156, and 169.)

4.165 cubic feet, the volume described by the pistons per double stroke, × 54.51 double strokes per minute = 228.12 cubic feet of steam consumed per minute; which, ÷ 0.517 cubic feet of water, evaporated per minute = 441 to 1, or ratio of the volumes of steam, and water. By referring to the table, it is found that 64 lbs. pressure per square inch, is the elastic force of steam ascribed to this ratio; deducting from which 14.71 lbs., we obtain 49.29 lbs. as the pressure per square inch, on the pistons, above the atmosphere.

Table X. contains the reduction of each experiment to terms of horses power, and exhibits under that denomination, 1st, the absolute power resulting from the steam used; 2dly, that required to overcome the assigned resistance; 3dly, their difference; 4th, the power which balances the gross, and useful duty. The consumption of water as steam, and of coke, per hour, and per horse power, under the above respective heads is also given; which, with the assistance of the remarks in the sequel, will enable us to compare the expenditure of power, with the effects produced according to the data of the different experimenters; and, finally, to test the accuracy of these data, by bringing the performance of the locomotive engines, for equal expenditure of power, into comparison with other engines in Table VI. The horse-powers have been computed as follows: example, Atlas, Experiment I.

For columns 33 and 34 the results are thus found; viz. for column 33;

Pressure: lbs. per square inch.		Area of the pistons: inches.		Length of double stroke: feet.		No. of double strokes per min.		Horses power.
49.29	× (	226	×	2.66	×	54.51	) =	48.93
				33000 lbs.				

and in like manner, for column 34.

For the gross duty (column 36);

Effect in lbs. raised one foot.		Minutes.		lbs.		Horses power.
257,813,952	÷	182, and again by		33000	=	42.92

and in like manner for the useful duty (column 37).

Column 38 shews the difference in horses-power between the whole power of the steam, and that required to overcome the resistances and frictions assigned, in Experiments I. to XII.; and this difference, according to the data of the experimenters, is the power consumed in blowing the fire.

The quantities of water and coke, in Table X., are the whole amounts actually expended by each engine, excepting when hereafter specially mentioned as corrected for ascertained waste: the term, "effective horse-power," column 35, will be explained in the sequel.

These three Tables—the columns of which are numbered forward, for more convenient reference, as if composing one continuous Table—thus present several modes of comparing the respective results of the experiments with each other, and with those of other classes of engine. For this purpose, the horse-power deduced from the gross duty will be preferable to the duty itself, as velocity is involved in the expression; and I propose, first, to apply the weight of water as steam expended in the production of that horse-power, as a measure of economical effect, and as a test of the accuracy of the data of resistance assigned by the different experimenters.

It is necessary to premise that the space passed over by the engines, in all the experiments, is presumed to be on a level plane. M. de Pambour * for Experiments I. to X., has adopted two measures of the distance travelled by his engines on the Liverpool and Manchester railway, in order to obtain a true comparison between them. When help was given by one or more engines in ascending inclines, he considers that help to have balanced the additional resistance of the acclivities, and the railway, consequently, to have been a dead level for the whole $29\frac{1}{2}$ miles traversed; and, when help was not given, he calculates the additional resistance, as equivalent to 5 miles on a level. In those cases, therefore, in which the train was unassisted, the space passed over is called $34\frac{1}{2}$ miles, and, when assisted, $29\frac{1}{2}$ miles. Thus, the velocities are calculated, in each experiment, from the distances assigned by the author, the time remaining unchanged, or that actually occupied by the trip. This is a rude method, but it answers the purpose for relative comparison, and it is the less necessary to shew its erroneousness, as it will hereafter appear that, in a later Work, M. de Pambour has selected for investigation, experiments on one of the engines in the Table, to which he has applied a new velocity, and revised data of resistance.

In column 11, the entire consumption of water is given, and from it the sums are derived in columns 39 and 41 to 45; the duty and the power are thus estimated by the whole weight of water as steam actually expended in each instance; but, the author informs us, (p. 184,) that we must deduct $\frac{1}{4}$th for waste, as only $\frac{3}{4}$ths of the water evaporated entered the cylinders, as steam. For relative comparison, therefore, the sums suffice, but they must, in each case, be diminished by one fourth in order to ascertain the effective performance of the steam, and to compare it with the engines in Table VI. The *effective*

* Treatise on Locomotives, pp. 323 to 325.

horse-power in the latter Table excludes, as does the *tractive*, or gross duty horse-power, (column 36,) all consideration of the friction of the engines unloaded, or loaded, and the comparison holds good, in both, for the expenditure of steam to produce equal effects, with the correction of $\frac{1}{4}$th for the locomotives.

Taking Experiments I. and II., (column 44,) we find that according to M. de Pambour, $47\frac{1}{2}$ and $50\frac{3}{4}$ lbs. of water as steam, only, have been consumed to generate a horse's-power, or to raise 33000 lbs. one foot per minute. The first result is perhaps within possibility at the pressure, but the second is impossible with a pressure of 29.79 lbs. per square inch, as, at that pressure, the expenditure of steam by the common non-condensing engine for a given power exceeds that of the rotative condensing engine, and we know the latter to consume 70 lbs. of water as steam for its maximum effect (Experiment V., Table VI.). It appears also, (column 31,) that a *vacuum* existed on the opposite side of the pistons in both these experiments; which is, also, an impossible result: we must, therefore, conclude either that the resistance of 8 lbs. per ton assigned to the load at 15 miles per hour is considerably too high, or that the evaporative data are exceedingly incorrect.

Experiment VIII., on the *Leeds* engine, satisfies the conditions of the non-condensing principle in the consumption of steam, at the pressure of 13.66 lbs., (as determined by Experiment III. in Table VI.,) inasmuch as, at the velocity of 26.70 miles per hour, the gross load being 44.08 tons, the horse-power required 123 lbs. of water as steam to create it. Thus, at that velocity, and with that load, 8 lbs. per ton was the sum of the entire resistance overcome by the steam, exclusive of the friction of the loaded engine. But the whole of this resistance must not be confounded with, nor considered as, tractive force; it includes the amount, whatever it might be, required to overcome the surplus pressure from the blast above the atmosphere, which must be ascertained, and deducted, before the true power, and the true consumption of water as steam, for tractive effort, can be determined. On this head the author was silent, and as before shewn, in explaining the Tables, I have endeavoured to discover the intensity of the blast-pressure, by ascertaining, and comparing the total pressure of the steam on the pistons, with that arising from the assumed resistances.

It appears, from the ratio of the volumes of steam and water consumed in this experiment, that the total pressure on the pistons was 25.62 lbs. per square inch, of which, the force required to overcome the sum of the resistances was 13.66 lbs., and their difference 11.96 lbs. per square inch; so that

this difference, or blast-pressure was equal, within $\frac{1}{8}$th, to the whole pressure required by the friction of the loaded engine, and the resistance of the engine, tender, and train: and this blast-pressure reduced to power (column 38) was 27.41 horses, whereas, the tractive force, from the datum of 8 lbs. per ton, (column 36,) amounted only to 25.11 horses-power.

A superficial examination would thus have indicated the resistance to traction of 8 lbs. per ton as correct, under the conditions of this experiment; but, such a conclusion would be most erroneous, for the water, as steam, has not only been employed to do the duty, but to blow the fire, which two operations have required $25.11 + 27.41 = 52.52$ horses power; and the water hourly expended as steam 3087.29 lbs. $\div$ 52.52 horses power $=$ 58.78 lbs. consumed per effective horse power, per hour, instead of 123 lbs. before found.

The enquirer will naturally ask whence proceeds the error, for some great error must exist, as it is not credible that, when the sum of the active resistances amounted only to 13.66 lbs. pressure per square inch on the pistons, the passive, or blast resistance should nearly equal it. The enigma is readily solved by comparing these results with Experiment VII. on the same engine; and the conclusion is inevitable that either the resistances, or the consumption of water, or both of these data, are very incorrectly estimated, for we find (column 31) a nearly equal blast-resistance resulting from the use of 39.19 and 25.62 lbs. absolute pressure on the pistons, respectively, in the two cases.

The *Vesta*, Experiment V., having the highest velocity in the Table, viz. 31.60 miles per hour, is represented to have had an absolute pressure of 15.30 lbs. per square inch on the pistons; the difference between that, and the pressure deduced from the resistance being only 0.62 lbs., which represents the blast-pressure.

The absolute pressure on the pistons corresponds within 0.30 lbs. with that in Experiment III., Table VI.; but the water consumed as steam, (column 44,) diminished by $\frac{1}{4}$th for waste, is only 101.55 lbs. for the effective load per horse-power per hour by the locomotive; whereas, the fixed non-condensing engine, without a blast, required 120 lbs. per horse power per hour.

The *Vulcan*, Experiment VI., has an absolute pressure of 20.04 lbs., being the same as the non-condensing engine, Experiment II., Table VI.; and its consumption of water as steam is only 99.86 lbs. for the same effect which required for its performance by the latter 125 lbs. The former, too, had a counter resistance against its piston from the blast equal to 5.94 lbs. per square inch, equal in power (column 38) to 11.38 horses, which, being so much load in

addition to the duty done, at 8 lbs. per ton tractive effort, brings the effective power of the steam in the *Vulcan* to 38.57 horses, and the consumption of water per horse power per hour to 70.40 lbs., or equal to the condensing engine—an impossible result at the pressure assigned.

The errors, therefore, are great and manifest. Had the consumption of water as steam been accurately given for Experiment VIII., the conclusion would have been indisputable, that 8 lbs. per ton was the whole resistance overcome by the steam besides the proper friction of the loaded engine; but this would not coincide with M. de Pambour's determination that 8 lbs. per ton was the tractive effort required of the engine; for, it includes the resistance opposed by the blast; so that, even at a velocity of 26.07 miles per hour, the resistance to progressive motion is overrated at 8 lbs. per ton. Thus, it is palpable that no reliance can be placed on the datum of water; and it is equally evident that the datum of resistance is very inaccurate; so that nothing can be gathered from this analysis, but that the author has signally failed both in his observations of the steam generated, and in his estimations of the force opposed to progressive motion.

Recurring to the experiment with the *Vulcan* at 26.90 miles per hour, I have already shewn the resistance to have been greatly exaggerated, the evaporation greatly underrated, or that we must be credulous enough to believe the horse-power to have been generated in the locomotive by 70.40 lbs. of water as steam per hour, using the same pressure which requires 125 lbs. in the fixed non-condensing engine to produce a like effect, 70 lbs. being the consumption of the condensing engine. Results equally inconsistent with the capabilities of the locomotive are perceptible in every one of these experiments.

I cannot imagine any conclusion to be safer, or more irrefragable, than that a condensing engine placed on wheels, with water of condensation transported for its supply, and made to drag a train of wagons, or coaches along a railway, would require precisely the same power, in water as steam, to produce a given effect, as if its foundations were based on a rock. No change has taken place in the engine by rendering it locomotive; it is intrinsically, and practically the same machine; and like effects would necessarily result from a like expenditure of power, whether the engine were at rest, or in motion: consequently, the effect produced, of whatever nature it may be, is measurable by the expenditure of power. By parity of reasoning, the non-condensing engine is one and the same machine, whether fixed, or locomotive; excepting

that the latter must consume more power than the former to do equal work, at like pressures, by the amount of additional resistance arising from the contraction of its eduction pipes, in order to produce a fierce blast of steam through the chimney. No change in mechanical structure has been made, but, it is reasonable to conclude that, from its shorter stroke, shorter connecting rod, and greater velocity of its piston, at high speeds, the locomotive would be subject to greater friction than a fixed engine; which would necessarily augment its consumption of power. The crank-shaft, too, through which the power is transferred, is liable to strains, and shocks, from which the same part of a fixed engine is free; hence, another disadvantage, and additional waste of power. Thus, the fixed non-condensing engine is the more economical of the two; but, if M. de Pambour's data are correct, we must abandon all preconceived opinions, and all belief in the accuracy of preascertained results on the non-condensing engine; we must reverse our engineering creed, and acknowledge the fixed non-condensing engine, with its simple atmospheric resistance, to be far inferior, in economy of steam, to the locomotive, with its plus atmospheric resistance.

It is seen that, according to the author, the *Atlas* in the two first experiments, consumed $47\frac{1}{2}$ and $50\frac{3}{4}$ lbs. of water as steam (waste deducted) per horse-power per hour, (column 44,) which gross amounts have still to be reduced for the power consumed in overcoming the blast pressure. We shall now see results more surprising than the foregoing, for it appears that though the resistance required 49.77 lbs. pressure per square inch on the pistons, in the one case, and 34 lbs. in the other, to overcome it, there existed no counter pressure in the blast pipe; but, on the contrary, a *vacuum*, (column 31,) amounting in the first experiment to nearly $\frac{1}{2}$ lb., and in the second, to $4\frac{1}{4}$ lbs. per square inch; column 38 shews, consequently, that less power was created by the steam, in these two experiments, than balanced the assigned resistances, amounting in the second case to no less than 6.45 horses-power. These are the results flowing from the comparison of the absolute pressure and power deduced from the ratios of the volumes of steam to water, with the power and pressure deduced from the respective sums of resistance assigned by the author. Hence, we are called upon to believe the authenticity of data which, in the case of the *Atlas*, Experiment II., establish a resistance equal to a pressure of 34 lbs. per square inch, with a *vacuum in the blast pipe;* and, in the case of the *Leeds*, Experiment VIII., a resistance equal to 13.66 lbs. on the piston, and *a counter pressure of* 11.96 *lbs. per square inch in the blast pipe;* we

are required to believe that, with an active pressure on the pistons $2\frac{1}{2}$ times greater in the one case than the other, the passive pressure against the pistons is $2\frac{1}{2}$ times less.

Let it not be thought that these cited cases are exceptions to the accuracy of others.

The *Vesta*, Experiment V., having a velocity of 31.60 miles per hour, exhibits a blast pressure of only $\frac{62}{100}$ths of a pound per square inch; whilst the *Vulcan*, Experiment VI., at 26.90 miles per hour, shews 5.94 lbs. per square inch as the counter resistance; and this last, whose velocity is the same as the *Leeds*, Experiment VIII., appears to have only half the amount of blast resistance to surmount, as the *Leeds*. Similar discrepances appear in all of them. It is however, preferable, and fairer to examine the authenticity of the data by results on the same engine. Thus, we should conclude, from Experiments I. and II. on the *Atlas*, that the resistance occasioned by the blast diminishes with the velocity, and with the active pressure; whereas, by comparing Experiments II. and III. on the same engine, a great increase in blast pressure is shewn at the higher velocity. In the *Leeds* the counter resistance is the same at two very different velocities, and two very different active pressures; in the *Fury* (the experiments on which engine will hereafter be shewn to be very anomalous) the blast pressure diminishes with the velocity of the piston, whilst the active pressure increases at the higher velocity. These discrepances arise out of extreme inaccuracy in the data of water, and resistance; and possibly, of velocity also.

A few more remarks on this subject are still requisite to render strict justice to the author. The previous comparisons between the intensity of pressure on the pistons, arising from the data of water, and resistance, result from a close adherence to his own instructions respecting the mean allowance for waste steam. This mean was deduced from special experiments on the quantity of steam which escaped during the trips, as determined by the degree of the rising of the safety valves, in each instance; and, though I attach no faith to the exactness of the approximations to waste obtained by this method, and adopted by the author, still it may be urged that each experiment should be investigated, after corrections suited to its own particular case.

The rising of the valve*, Experiment I. on the *Atlas*, indicated a loss of $\frac{1}{8}$th of the steam, so that, to arrive at perfect correctness, we must deduct that

* Treatise on Locomotives, p. 75.

quantity instead of $\frac{1}{4}$th. Using, therefore, this correction for water consumed, and afterwards $\frac{1}{20}$th for waste steam, the ratio of steam to water is 390 to 1, and the pressure on the pistons 58.10 lbs. per square inch, which, by comparison with the pressure due to the ascertained sum of the resistances, would give 8.33 lbs. per square inch for the unascertained amount, or blast pressure. This result would seem to indicate an approach to the region of truth and credibility; but, unfortunately, a reference to column 32 destroys the hope of so easily extricating ourselves from this labyrinth of error, by exhibiting the fact that the pressure in the boiler (about which I should think there could be no mistake) was only 53.7 lbs., so that the pressure on the pistons could not have exceeded it, or have attained 58.10 lbs.! Granting, however, for an instant, that the pressure in the boiler might have been erroneously observed, and that the data for the resistances and water were, possibly, correct, let us see how the latter accord with the results of other engines. The gross duty or tractive power (column 36) is 42.92 horses, and the power required to overcome the blast pressure of 8.33 lbs., now found, would amount to 8.26 horses; so that the effective power of the steam was 51.18 horses. The water consumed per hour was 2703.07 lbs. − 453.84 lbs., for $\frac{1}{6}$th wasted, = 2269.23 lbs. which ÷ 51.18 horses = 44.33 lbs. of water as steam consumed per horse-power, per hour; so that the *Atlas*, in this experiment, did equal work with the non-condensing engines in Table VI., with little more than one third, and equal work with the rotative condensing-engines with little more than two thirds the respective expenditure of water, as steam, by those engines.

I will take one more example from the *Atlas*, viz., Experiment II., (the 2d, also, in the author's Treatise before referred to, p. 75). The rising of the valve indicated an actual loss of $\frac{1}{40}$th only; deducting, therefore, this quantity from the water, instead of $\frac{1}{4}$th, and afterwards $\frac{1}{20}$th, the ratio of the volumes of steam and water comes out as 472 to 1, and shews a pressure of 44.04 lbs. per square inch on the pistons, from which subtracting 34 lbs. required for resistance, 10.04 lbs. would be left for blast pressure. Now, the gross duty, or tractive power is (column 36) 44.65 horses, and the blast pressure is equivalent to 15.37 horses-power, making 60.02 effective horses-power. The whole water consumed per hour was 3018.81 lbs. which, diminished by $\frac{1}{40}$th = 2943.34 lbs. ÷ 60.02 = 49.03 lbs. as the consumption of water as steam per effective horse-power, per hour; which is, also, an impossible result.

I have thus applied the author's most minute corrections to his experiments; I have "traced the use of every pound of steam, in the effect produced," according to his own data; and the result is, that if those data be correct, the sooner we substitute non-condensing, for condensing engines, for all purposes, the better; for, it seems that the locomotive has a faculty of producing an equal effect, with only two-thirds of the expenditure of power required by the condensing engine.

The foregoing investigation of M. de Pambour's experiments was completed previous to the appearance of his "*New Theory of the Steam Engine*," but I have since computed all the pressures resulting from the ratio of the volumes of steam and water, by the elaborate and useful Table contained in this last work, which I have transferred to the end of these pages as necessary for reference. These pressures were originally deduced from a less ample, and, probably, less accurate table in the same author's "*Treatise on Locomotives*," page 204.

M. de Pambour has seen reason, since the first publication of his experiments, to modify his original data of resistance, by assigning relative quantities due to friction, and to the air, at all velocities. He has, also, supplied his original omission of blast pressure, by assigning a value for it, at all velocities. He has, thereby, enabled me to examine his revised opinions as contained in his "*New Theory of the Steam Engine*"; and it will presently be seen whether, and in what degree, he has corrected the errors pointed out in the experiments themselves, or in his deductions from them. The new, or amended data adopted by the author are;

1st. That, "when the velocity is 10 miles per hour for the engine, or 150 feet per minute for the piston, the pressure arising from the blast-pipe is 1.75 lbs. per square inch on the surface of the piston, and that it varies in the direct ratio of the velocity of the motion." (page 161).

2d. That, "the resistance of the air against a train of mean surface is 33 lbs. at the velocity of 10 miles per hour for the engine;" and "that it increases as the square of the velocity." (page 162).

3d. That, "the resistance to progressive motion, independent of the air, is 7 lbs. per ton." (page 35).

Using these data, the author has adduced two of his previous experiments on the *Leeds* engine, to shew the superior accuracy of his *New Theory*, over the theories of other writers on the subject. With these I have nothing to do, dealing, as my object professedly is, only with facts, or with data

assumed as facts, which I shall proceed to quote in the author's own words, referring to the number of the experiment corresponding with them in Tables VIII., IX., X. The following particulars are extracted from pages 34 to 41, "*New Theory of the Steam Engine.*"

Experiment XIII. "The locomotive *Leeds*, which has two cylinders of 11 inches diameter; stroke of the piston, 16 inches; wheels, 5 feet; weight, 7.07 tons; drew a load of 88.34 tons, ascending a plane inclined $\frac{1}{1300}$, at a velocity of 20.34 miles per hour, the effective pressure in the boiler being 54 lbs. per square inch, or the total pressure 68.71 lbs. per square inch."

Experiment XIV. "The same day, the same engine drew a load of 38.52 tons, descending a plane inclined $\frac{1}{1094}$, at the velocity of 29.09 miles per hour; the pressure in the boiler being precisely the same as in the preceding experiment, and the regulator opened to the same degree. These experiments are given, pp. 233, 234, of our *Treatise on Locomotives.* (1st Edition.)"

"Now, during the journey, of which the first of these experiments was a part, the engine evaporated 60.52 cubic feet of water per hour; (*Treatise on Locomotives*, 1st edition, p. 175;) which, after a deduction of $\frac{1}{5}$th for the loss of steam by the safety valve, measured as explained in Section VII., and of $\frac{1}{20}$th on the rest for the filling up of the vacant spaces of the cylinder, leaves an *effective* evaporation of .77 cubic feet of water per minute." The same evaporation is applied by the author to the 2d experiment.

He then proceeds to give "a detailed calculation of the effects produced" in the two cases, which I shall follow verbatim, separating only the friction and resistances of the engine, and tender, from that of the train, with the view of ultimately exhibiting the power usefully, and uselessly expended by a locomotive. In this work the author refers for an explanation of his present use of 7 lbs. per ton as the tractive resistance, and of his method of ascertaining the blast pressure, and air resistance, at different velocities, to the "2d edition of his *Treatise on Locomotives;*" but it is remarkable that this 2d edition, quoted by him as a necessary reference, has not yet seen the light; so that I am unable to elucidate these points.

DETAILED CALCULATION OF THE EFFECTS.

1st Case. (Experiment XIII.)

		lbs.	lbs.
Resistance of the train (20 wagons) 83.34 (Tons), at 7 lbs. per ton		583	
Gravity of 83.34 tons, on plane rising $\frac{1}{1300}$		+143	
			726
Resistance of the air against the train at the velocity of the motion			134
Friction of the engine, without load	82 (lbs.)		
Additional friction for a resistance equivalent to $\frac{916}{7} = 131$ tons	131		
		... 213	
Resistance of the tender 5 tons, at 7 lbs. per ton		35	
Gravity of engine and tender 12.07 (Tons) on the plane		+ 21	
			269
For pressure caused by the blast-pipe, being at the velocity 3.4 lbs. per square inch on the piston			109
Total resistance overcome at the velocity of the wheel			1238

$$\therefore 1238 \times 5.9 \div 190.06 = 38.43 \text{ (lbs. per sq. in. on piston) and } 38.43 \times \left(\frac{190.06 \times 2.66 \times 114}{33000}\right) = 67.11 \text{ (horses power).}$$

2d Case. (Experiment XIV.)

		lbs.	lbs.
Resistance of the train (7 wagons) 33.52 (Tons), at 7 lbs. per ton		234	
Gravity on plane descending $\frac{1}{1094}$		− 68	
			166
Resistance of the air against the train, at the velocity of the motion			282
Friction of the engine, without load	82 (lbs.)		
Additional friction for a resistance equivalent to $\frac{459}{7} = 66$ tons	66		
		148	
Resistance of the tender, 5 tons, at 7 lbs. per ton		35	
		183	
Gravity of engine and tender 12.07 (Tons), on the plane		− 24	
			159
For pressure caused by the blast-pipe, being at the velocity 5.1 lbs. per square inch on the piston			164
Total resistance overcome at the velocity of the wheel			771

$$\therefore 771 \times 5.9 \div 190.06 = 23.93 \text{ (lbs. per sq. in. on piston.) and } 23.93 \times \left(\frac{190.06 \times 2.66 \times 162.41}{33000}\right) = 59.50 \text{ (horses power.)}$$

These data are elaborated in the Tables to the same terms as the other experiments, but with certain changes of value arising out of the assigned aliquot parts of the total resistance, which require notice; and this is the more necessary, because the accuracy of the author as an experimenter, together with the practical value of his experiments and deductions, are to be judged of by these new data, rather than by the foregoing investigation of his old data; inasmuch as they must be considered to be his conclusions matured by four years of reflection, and additional experience. By the subdivision of the frictions and resistances, *duty* can be properly estimated—as, also, *effective horse-power*—so as to bring these definitions of work and power, to a truer comparison with the same definition applied to other engines. I must, therefore, shew the manner of separating, and ascertaining these values, as respectively set down in the columns.

I consider the *duty* of a locomotive to be that appreciable effect which is performed by the engine independent of all opposition from the air to the progressive motion of a train. It is only in this view that duty strictly corresponds with its received definition; for the practical performance of a pumping engine estimated by the duty done, or weight of water raised one foot, by a pound of steam, omits taking into account the friction of the water in the pipes; the duty is taken on the weight raised, only. The weight raised by a locomotive, in the same sense of duty, is that which is necessary to communicate motion to the train; and this weight, or tractive force, is now assumed by the author to be equal to 7 lbs. per ton of matter moved on a level plane, and to be a constant quantity, at all velocities. In the cases cited, he increases, or diminishes this weight, in ascending or descending inclines, on the principle that the tractive force "is the gravity along the plane, and equal to the mass that is to be moved, divided by the number that marks the inclination of the plane." (*Treatise on Locomotives*, p. 206).

The true *effective power* of the steam is here considered to be that expended in overcoming every species of resistance, excepting the friction of the engine itself, and the counter pressure of the blast. It is not in every case that this amount can be arrived at, as previously shewn, (page 51 of this paper,) but according to the data before us it is determinable. It is the gross duty just explained, plus the resistance of the air. The proportionate values of these several resistances are thus found:

1st Case. (Experiment XIII.)

Tons.		lbs.		lbs. per ton on the gross load.
Resistance of train and tender 88.34, at 7 lbs. per ton		618		
Gravity of 95.41 tons (engine, tender, and train) on plane rising $\frac{1}{1300}$		+164		
Tractive force for computing the gross duty	At the velocity of the wheel	782	=	8.19
Resistance of the air, at 20.34 miles per hour		134	=	1.41
Tractive force for computing effective power.	At the velocity of the wheel	916	=	9.60
	Friction of loaded engine	213	=	2.23
	Resistance from blast-pipe	109	=	1.14
Total resistance at the velocity of the wheel		1238	=	12.97

2d Case. (Experiment XIV.)

Tons.		lbs.		lbs. per ton on the gross load.
Resistance of train and tender 38.52, at 7 lbs. per ton		270		
Gravity of 45.59 tons descending plane $\frac{1}{1094}$		−93		
Tractive force for computing gross duty.	At the velocity of the wheel	177	=	3.88
Resistance of the air at 29·09 miles per hour		282	=	6.72
Tractive force for computing effective power.	At the velocity of the wheel	459	=	10.60
	Friction of loaded engine	148	=	3.24
	Resistance from blast-pipe	164	=	3.07
Total resistance at the velocity of the wheel		771		16.91

Supposing these data to be correct, the decomposition of the component parts of the total resistance enables us to determine the amount of steam power consumed in overcoming each item of resistance, at the two velocities; and I have, accordingly, worked out the particulars as follows:

	Exp. XIII. Velocity 20.34 miles per ho. H. P.	Exp. XIV. Velocity 29.09 miles per ho. H. P.
Resistance of the train	39.355	12.810 Useful effect.
Ditto of the air	7.263	21.762
Ditto of engine and tender, and friction of the engine.	14.582	12.270
Ditto caused by the blast	5.908	12.656
Absolute power of the steam	67.108	59.498
Effective ditto	49.654	35.422
Ineffective ditto	17.454	24.076
Useful power of the engine	39.355	12.810
Useless ditto	27.753	46.688
	67.108	59.498

The *useful effect* is the useful duty arising from the resistance to the train only, and it thus appears from these two experiments (always premising the data to be correct) that at a velocity of 29.09 miles per hour, it required more than three times as much power to realize the same useful effect, as at 20.34 miles per hour; a result, of which we shall, hereafter, see great reason to doubt the truth.

In order to establish a true comparison between these new results and the engines in Table VI., column 16, the whole water must be divided by the sum of the effective power, plus the additional power required to balance the blast resistance; these amount for the 1st case to $55\frac{1}{2}$, and for the 2d case to 48 horses, which gives the water consumed respectively per horse, per hour, as $54\frac{1}{2}$ and 63 lbs. In all the columns the water has been diminished by $\frac{1}{5}$th for waste according to the author's instructions. Thus, it is evident that the original error of far too high an estimate of resistance still clings to the data as amended, for it is utterly impossible that the locomotive should accomplish an equal effect, with $\frac{1}{5}$th less steam than the condensing-engine. To go over this ground again, would be a mere repetition of arguments previously used.

It is also demonstrable that, on the supposition of the data being correct for the first, they are incorrect for the second experiment; for the author has left nothing to be guessed at; no unknown quantities have to be found from known ones; he has assigned the particular values of every component portion, as well as the sum total of the resistances to be overcome. He has, also, informed us that in the two experiments "the pressure in the boiler was precisely the same, and the regulator opened to the same degree." The power

applied in the two cases was, consequently, precisely equal, and equal weights of water as steam passed through the cylinders in equal times; whence, it results that the effects should have been similar. The expenditure of power was, however, greater by more than a third in the second than in the first case, to produce like effects, for we see that the effective horse-power required 85.43 lbs. of water as steam in the second, and only 60.94 lbs. in the first. Either, then, the sum of the resistances overcome must have been greater by one third, than the author's estimate, in the 2d experiment, or it was too small in the 1st, or he has erred in applying the same measure of water to both.

It is very remarkable that such an enormous discrepancy should have escaped M. de Pambour's notice. In his *Treatise on Locomotives*, pp. 310 to 312, he states a near parallel to these two experiments by supposing a case of "the same engine, with the same pressure in the boiler, travelling the same distance with two different loads. The distance travelled being the same, the number of turns of the wheel, and consequently of strokes of the piston, or cylinders of steam expended will be the same in the two cases;"—he proceeds, "the mass, or weight of steam expended, will be in each case in the ratio of the pressure in the cylinder. The weight of the steam being equal to that of the water that generated it, the weights of water evaporated will then be to each other as the pressures in the cylinder, or, in other words, as the resistances on the piston." In the "*New Theory of the Steam Engine*," the author has dwelt at great length, and occupied many pages, to prove this indisputable, and, so far as I know, never disputed fact; viz. that the steam, in every engine, necessarily assumes in the cylinder, the pressure due to the resistance it has to overcome; or, to use his own phrase, that the pressure of the steam, and the resistance "*equilibrate*." Now, the author has given us the resistances on the piston, which amount in the 1st case to 38.43 lbs., and in the 2d, to 23.93 lbs. per square inch; and yet he assumes an equal expenditure of water as steam, in equal times, in the two cases, and applies it (Chap. I. pp. 43, 44, *New Theory of the Steam Engine*) to demonstrate the truth of his own theory, and the inaccuracy of other theories. To be consistent, however, with his own rule, above quoted, viz. that "the weights of water consumed as steam are to each other as the resistances against the piston," it is obvious that if, in the 1st case, 3026 lbs. of steam passed through the cylinders in an hour, 2166 lbs. only would have been expended in the 2d case.

The expenditure of water as steam would have been equal, in equal times,

or alike in both these cases, had the resistances been alike; but it is evident that the formulæ applied to the estimation of the latter, are insufficient for the solution of the problem, for if, as asserted, "the pressure in the boiler were precisely the same, and the regulator open to the same degree," in the second, as in the first experiment, equal power must have been generated and expended in the same time; though, at the higher velocity, the lighter load was moved through a greater space in that time.

Had M. de Pambour reduced his data to the terms of value in these Tables, he must inevitably have discovered the numerous errors of fact, and deduction, which are now brought to light; had he worked out the arithmetical computations of which his formulæ are merely symbolical, and compared their results with the performance of the Albion Mills engine alone (which he has analysed in his "*New Theory*"), he would have seen reason to doubt the accuracy of his experiments.

I have already commented upon the nice and numerous precautions requisite to give accuracy to the method of finding the pressure on the piston from the ratio of the volume of steam consumed to that of the water which produced it. I am disposed to believe this test to be as perfect as it is beautiful; I entirely agree with the author in his estimation of its practical value; but I will now adduce a most remarkable instance of its coincidence with error of resistance, as well as an instance of its utility in detecting error.

By referring to columns 29 and 30, Experiment XIII.,—the first of the two cases under review—it is seen that the pressures on the piston deduced by the method of the volume, and from the sum of the assigned resistances are identical; yet we know one or both of these data to be incorrect, and greatly so, since an equal power is exhibited as having been produced by $\frac{1}{3}$th smaller consumption of steam than the best specimen of the condensing engine, and by only one half the necessary expenditure of the fixed non-condensing engine. Had we, therefore, no other means of ascertaining the accuracy of an assigned expenditure of power than that afforded by the comparison of the volumes of steam and water, the volume test would, in this case, have confirmed a great error in the datum of resistance, for the errors of water, and the errors of resistance coincide.

By referring to the same columns, Experiment XIV., we find the pressure per volume insufficient to balance the resistance, and that the whole power, indicated by the volume test, falls short of the required amount, by nearly 6

horses power. Supposing the resistances assigned to have been correct, the ratio of the volume of steam to that of the water consumed, should, in this case have been 700, instead of 741 to 1, and .81 cubic foot of water must have passed through the cylinders in the shape of steam, per minute, instead of .77 cubic foot, to have impressed upon the pistons a force of 23.93 lbs. per square inch, which is the pressure deduced from the resistance.—But, we have already seen that, if the quantity of water were correctly taken in the 1st case a less quantity must have been consumed in the 2d, as the load upon the pistons of the engines in the two experiments deduced from their velocity and assigned resistances, differed in the ratio of 38.43 to 23.93; and the water as steam consumed in equal times must necessarily have varied in the same ratio, or as 3026 lbs. to 2166 lbs. The pressure, therefore, deduced from the corrected datum of 2166 lbs. for the 2d case, after deducting $\frac{1}{20}$th for waste, should accurately balance the resistance; but dividing 2063 lbs. (the sum last found) by 60 minutes, the duration of both experiments, 0.55 cubic foot is the consumption of water per minute, and 1037 the ratio of the volumes of steam and water; whence, from the author's Table, the pressure per square inch on the pistons would be $24.81 - 14.71 = 10.10$ lbs. only; whereas, the resistance required 23.93 lbs. pressure per square inch to balance it.

The author might object to the comparison of steam consumed by the effective power, and consider the proof of accuracy to be more justly dependent on the relative consumption of steam by the absolute power of each engine.

The absolute power in the two cases, is as 67.24 to 53.55 (column 33,) and the expenditure of water as steam by this ratio should have been as 3026 to 2409 lbs.; and the latter corrected for waste, would give 935 to 1 as the relative volumes of steam and water $= 28.20 - 14.71 = 13.49$ lbs. per square inch on the pistons, whilst the resistance was 23.93 lbs.!

It would be fruitless to pursue this analysis further, and vain to attempt the rectification of errors—a task which properly belongs to the author. It is manifest that, in both these cases, as well as in the former series, resistances have been assigned which the force of the steam employed was insufficient to overcome; it is manifest that the sum of the resistances at the different velocities—whether arising from erroneous estimations of the friction of motion, friction of the engine loaded, and unloaded, resistance of the air, or pressure occasioned by the blast—is purely hypothetical, and has no foundation in fact; and, it is manifest that the evaporative data are extremely fallacious.

To arrive at synthetical results was the chief object of M. de Pambour's analytical labours; for, the intrinsic value of his labours depends on the accordance established between the theoretical and practical performance of the locomotive engine at various velocities, and with various loads: it is, therefore, not a little singular that this ingenious and truth-seeking experimenter should have omitted to bring the whole series of his experiments into juxta-position, and have compared his estimation of resistance with the expenditure of steam, as in Tables VIII., IX., X. He would then have discovered the little reliance to be placed on quantities deduced from mere theory, and have perceived the necessity of obtaining evaporative data from much more careful, and satisfactory observations.

Experiments by Mr. Robert Stephenson. The particulars of Experiments XI. and XII. are extracted from a separate publication of Mr. Robert Stephenson's elaborate article on the locomotive engine, contributed by him to Tredgold's work on the Steam Engine, second edition*.

* "The power of a locomotive engine cannot readily be estimated in the same manner as that of other engines, by taking the actual force upon the piston, and the velocity of its motion; for it is very difficult to ascertain the effective pressure of the steam upon the piston, in consequence of its differing often very considerably from that of the steam in the boiler, and because of the large amount of the resistance of the waste steam, owing to the great velocity with which the piston moves. The power is also different at different velocities, as these circumstances vary with the velocity. The only correct means, therefore, of ascertaining the power of a locomotive, is by deducing it from the work which it is capable of performing.

"This engine has drawn a load up an inclined plane that was equivalent to 220 tons gross weight upon a level, (including engine and tender,) at a velocity of 14 miles an hour; which appeared to be about the extent of the power of the engine with the steam at the usual pressure of 50 lbs. on the square inch, in the boiler. The force required to perform this, is about 2,050 lbs. moving at that velocity; which is equal to 77 horse-power. The effective pressure on the piston, or the actual force with which it was propelled, must therefore have been 47½ lbs. per square inch, instead of 50 lbs., which was the pressure of the steam in the boiler; the difference being the power that was lost by the resistance of the waste steam, and by the diminution of the pressure of the steam, in consequence of the throttling or wire drawing that takes place in passing through the ports of the cylinders, and which was in this instance very inconsiderable.

"The horse-power of an engine is less when drawing a lighter load at a greater velocity, as the loss of power from the throttling and the waste steam is then increased; and it would cease altogether at a certain speed, varying according to the proportions of the engine, when the velocity of the piston became as great as that with which the steam can enter into the cylinder, or the waste steam escape. This engine has drawn 40 tons at 35 miles an hour, which is equivalent to 40 horse-power; in which case the effective pressure on the piston must have been only 10 lbs. on the square inch, or but one-fifth of that of the steam in the boiler. This shews the great loss

The author has adopted M. de Pambour's original datum of resistance, viz. 8 lbs. per ton, for calculating the power of this engine at the two velocities cited. The fallacy of that estimate having already been demonstrated, it is unnecessary to submit these results to the same minute investigation, as in the previous instances; yet a few remarks are requisite to point out some remarkable discrepances between the power represented to be employed, and its estimated effect, in the two experiments.

It cannot be said that Mr. Stephenson has not reduced the data of resistance and velocity to terms of power, for he has discovered and stated that this locomotive, "when working with a full load, consumes a cubic foot of water as steam per hour for each horse-power," which, he observes, "is also the usual

of power in working the engine so quickly; the loss at 14 miles an hour having been very little. The loss of power is lessened by making the wheels larger, the velocity of the piston being by that means diminished; and they are consequently made as large as is practicable.

"The power of a locomotive engine is limited only by the evaporating power of the boiler, or the number of cylinders full of steam that can be supplied in a given time, by which the velocity of the piston is determined; while in other steam engines the size of the cylinder is the limit to the power, as a sufficient quantity of steam to supply them can be readily obtained by increasing the size or number of the boilers, which cannot be done in a locomotive. This engine is capable of evaporating 77 cubic feet of water per hour, or eight gallons in a minute; and the large amount of this power causes its great superiority to the old locomotives, which could evaporate only about 16 cubic feet per hour.

"The consumption of fuel per mile for every ton of the gross load is about a quarter of a pound, and that of the water is rather less than a quarter of a gallon; the consumption increasing to nearly one half, according as the engine is less fully loaded, being proportionally greater with a light load; the consumption of water when working with a full load, is a cubic foot per hour for each horse-power, which is also the usual proportion in stationary engines although they condense the steam. About 8 lbs. of fuel is required to evaporate a cubic foot of water, being nearly the same as in stationary engines; but in the old locomotives as much as 18 lbs. was required, in consequence of their having so small a heating surface, which was only about two and a half square feet for each foot of water evaporated per hour; the proportion in the present one being five and a half square feet, and in stationary engines as much as eight.

"The great perfection of the present locomotives, and their superiority to the old ones, is caused not so much by the application of new inventions to them, as by the combination of many former ones, and the uniting together several plans which, separately, would be but of small value. Their great power and velocity, for example, could not have been obtained without the rapid means of generating steam afforded by the use of the tubes; and the tubes would have been useless, without the powerful draught produced by the blast, which increases in intensity with the velocity, and with the necessity for its increased action."—(*Description of the Patent Locomotive Steam Engine, by Messrs. Robert Stephenson and Co.*, pages 66 and 67.)

proportion in stationary engines, although they condense their steam." He has also, advanced another still more extraordinary proposition, viz. that, with a lighter load, the same engine consumes nearly half as much more water as steam to produce an equal mechanical effect, as when fully loaded. These are paradoxes naturally resulting from the adoption of M. de Pambour's too high estimate of tractive force, and from considering the tractive force, *per ton* of matter moved, to be a constant quantity at all velocities. It is certainly singular that, with so clear a perception of the conclusions necessarily flowing from these data, and exhibited by his own computations, Mr. Stephenson should not have hesitated in adopting them; for, even if the locomotive were a condensing engine, it is evident that its effective power must undergo diminution by the amount requisite to move itself and its tender, a force which, even at the lowest velocity, is far from being inconsiderable. It is equally evident that a stationary non-condensing engine consumes more steam, for equal effect, than a stationary condensing engine; and that the locomotive, compared with the former, has not only to expend steam to move itself, and its tender, but, also, to overcome the resistance caused by the blast.

The author has given an excellent commentary on this obvious fact, for he states, in the third paragraph quoted, that "the horse-power of the engine would cease altogether at a certain speed." The meaning of that sentence is not very obvious, but I imagine it to be the author's intention to convey to his readers, that there is a velocity at which the whole power of the steam would be absorbed in moving the locomotive itself, and its tender; consequently, that the engine could drag no load at that velocity, with the identical quantity and pressure of steam then applied. There is, doubtless, a velocity at which the useful power of the *engine*, commercially speaking, ceases; nevertheless, the effective power of the *steam* is the same at all velocities of the engine, under all loads, for an equal consumption of water as steam; and, it will hereafter be shewn that an equal mechanical effect results, at all velocities, from an equal expenditure of power in the locomotive, as in all other engines.

In the fourth paragraph the author observes that "the power of a locomotive engine is limited only by the evaporative power of the boiler," &c. This definition is clearly imperfect, for no power of steam could impart progressive motion to the engine, if loaded beyond the force of its adhesion to the rails; that force is, therefore, the dynamic limit; the supply of steam is a mere practical condition of the boiler.

These two experiments supply additional means for disproving the accuracy of M. de Pambour's data; they likewise furnish powerful incentives to experimenters to be vigilant in obtaining precise observations of evaporative facts, for they contain within themselves, abundant proofs of error in the quantities assigned to the consumption of water as steam.

Experiment XI. The sum of the resistances distributed over the pistons (column 29) is 47.54 lbs. per square inch, being the same as calculated by Mr. Stephenson from M. de Pambour's data; the pressure in the boiler is stated at 50 lbs., "the difference (or $2\frac{1}{2}$ lbs.) being," according to the author, "the power that was lost by the resistance of the waste steam, &c., &c.—which was in this case very inconsiderable." This method of finding the blast pressure does not suffice, for the pressure in the boiler is totally irrespective of the force on the pistons; the resistance caused by the blast is the difference between the absolute pressure of the steam in the cylinders, and that required to balance the sum of the ascertained resistances. The blast resistance is the unascertained portion of the total resistance opposed to the steam, and is discoverable when the absolute force exerted by the steam on the pistons is known. Now, if the evaporative data are correct, it would appear by the ratio which the volume of steam consumed bears to that of the water which produced it ($\frac{1}{26}$th being deducted for waste) that the absolute pressure upon the pistons in this case amounted to 81.95 lbs. per square inch; but there was only 50 lbs. in the boiler! If, therefore, 77 cubic feet of water passed through the cylinders in an hour, in the shape of *pure* steam, the blast pressure, or counter effort above the atmosphere, was 34.41 lbs., instead of $2\frac{1}{2}$ lbs., per square inch on the pistons.

The author has given no instructions for diminishing the consumption of water as steam, for any issue through the safety valve, priming, or other waste; and if none of the steam were wasted, it must have exerted the above force on the pistons; but, perceiving an error, I applied M. de Pambour's correction of $\frac{1}{4}$th in computing the pressure per volume, (column 28,) and even with this allowance, it is seen that the absolute pressure amounted to more than $5\frac{1}{2}$ lbs. above that in the boiler, which is also impossible, if the boiler pressure be correctly stated. In order to make the pressure on the pistons coincide with that in the boiler, (to which, however, strictly speaking, it can never attain, in a locomotive,) $\frac{2}{7}$ths of the whole water said to be evaporated must be subtracted for loss, and the horse-power would then be performed, according to the datum of resistance, by 44.8 lbs. instead of by a cubic foot, per hour; and, then, there

is nothing left for blast pressure, with a presumed force of $47\frac{1}{2}$ lbs. per square inch in the cylinder.

Mr. Stephenson assumes, with M. de Pambour, that the resistance from the blast increases as the pressure on the piston diminishes. The probability of the correctness of this assumption will be hereafter examined. The volume test shews either that 77 cubic feet of water did not pass through the cylinders as steam; or that, if such were the case, a pressure of above 80 lbs. must have existed in the boiler; and, if so, the blast resistance must have exceeded 34 lbs. per square inch. The truth probably is that the boiler *primed* to a great and unobserved extent, and thus vitiated the experiment, both as regards evaporation, and the expenditure of power.

Experiment XII. In this case I have assumed an equal evaporation in the same time, as in the foregoing experiment; and if $\frac{2}{7}$ths were deducted for waste, the blast pressure would be less than nothing—or a vacuum; for, with the subtraction of $\frac{1}{4}$th for waste, as in the Table, the absolute pressure amounts only to 11.10 lbs., whilst the resistance required 10 lbs. per square inch: and if, contrary to demonstration, it be considered possible that the 77 cubic feet of water were converted into pure steam, and that this quantity passed through the cylinders in the hour, the blast pressure would equal the whole force required to balance the assigned resistance; for the absolute pressure on the pistons would have amounted to 20.70 lbs. per square inch, whilst the sum of the ascertained resistances was only 10 lbs.

Making an equal deduction of one fourth for waste of water, in the two cases, and $\frac{1}{20}$th for ineffective steam in the passages, the blast pressure would have been (as set down in column 30) 8.15 lbs. in Experiment XI., and 1.10 lbs. in Experiment XII., thus exhibiting, with an assumed equal consumption of water as steam, in equal times, the very reverse of the doctrine that the blast pressure is in the inverse ratio of the absolute pressure on the piston—or, in M. de Pambour's words, that it increases with the velocity of the piston. The data of resistance and evaporation are, however, proved to be so erroneous, that these experiments present no facts on which to ground either a measure of the force of steam actually expended, or a measure of the tractive effort actually exerted; consequently, no truthful approximation can be derived from them as to the value of blast pressure.

It is proper to notice that in Experiment XII., I have assumed the same consumption of water as in the first case, being unable to reconcile the author's

statement of evaporation with his deductions as to the quantity of water consumed per ton of gross load per mile. In the fifth paragraph quoted, he applies a cubic foot per hour per horse-power to the first experiment, and that power, from his data of resistance, being 77 horses, there is an accordance with the assigned evaporation of 77 cubic feet per hour. But, in the same sentence, he says that the consumption of water per ton per mile is something less than a quarter of a gallon, which on the load of 220 tons for 14 miles amounts to nearly 7,700 lbs. instead of 77 cubic feet or 4,812 lbs. as before stated; and 220 tons was very nearly the maximum load of that engine. In the second case the expenditure of water at nearly half a gallon per ton per mile, would amount to nearly 7,000 lbs. I have assumed 4,812 lbs., and shewn that consumption to have greatly exceeded possibility.

Experiments by Mr. Nicholas Wood. Mr. Wood's series of experiments with seven engines on the Great Western, and two engines on the London and Birmingham Railway, will be found of great use in illustrating another section of this subject; but though they embrace the question of expenditure of power, Mr. Wood has thrown no new light on the resistance opposed to progressive motion. He has, indeed, in his "Report to the Directors of the Great Western Railway," adopted Dr. Lardner's conclusions relative to the amount of resistance arising from friction, as well as from the atmosphere; but as Dr. Lardner has made additional experiments on these values, and has since furnished the means of testing their accuracy by a practical experiment on the Grand Junction Railway, it is needless to reduce Mr. Wood's to the terms of the Tables; I shall, therefore, pass to the consideration of the latter.

Experiments by Dr. Lardner. The 15 experiments recorded in the following Table were communicated by Dr. Lardner to the British Association for the Advancement of Science in August last. Their purport was to determine the resistance opposed to progressive motion on railways. The experiments consisted in dismissing trains at various speeds from the summit of inclined planes, and in observing their velocity when it became uniform; the resistance, at such velocity, being inferred to be equal to the force representing the gravity of the mass along the plane. Table XI. contains the particulars of the experiments together with the sum of resistance, or tractive force, computed from the author's data, and placed in the last six columns. The resistance is likewise separated into two amounts, the one supposed to balance atmospheric opposition, the other supposed to be the value of the friction of the axles, and the friction between the wheels of the

carriages and rails. These component portions of the total resistance are also reduced, in each experiment, to their proportionate values per ton of matter moved. I have been obliged to assume an amount for friction as the author fixed no positive quantity for it, but he considers it to be nearly a *constant* at all velocities, and though 6 lbs. per ton may be somewhat higher than his standard, as collected from his remarks at Birmingham, and his Report to the British Association, in Vol. VII., the subsequent investigation will shew that the separation of the sum of the resistance into quantities attributable to this or that source, is of no moment as regards the objects of this Paper.

TABLE XI.

Experiments.	Weight.	Number of coaches.	State of the wind.	Inclination of plane.	Uniform velocity acquired.	RESISTANCE OR TRACTIVE FORCE.					
						Total.	Per ton.	From the air.	From friction.	From the air per ton.	From friction per ton.
	Tons.			One in	Miles per Hour.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
1	15.6	4	Favorable.	96	31.20	364	23.33	270.4	93.6	17.33	6
2	18.0	...	...	...	33.72	420	23.33	312.0	108.0	17.33	6
3	18.0	...	...	177	21.25	227.80	12.65	119.8	108.0	6.65	6
4	20.5	...	...	...	22.90	259.43	12.65	136.43	123.0	6.65	6
5	20.5	...	...	89	38.25	515.95	25.16	392.95	123.0	19.16	6
6	20.5	...	...	265	19.13	173.28	8.45	50.28	123.0	2.45	6
7	27.5	6	Adverse.	89	32.30	692.13	25.16	527.13	165.0	19.16	6
8*	27.5	...	Favorable.	...	37.50	692.13	25.16	527.13	165.0	19.16	6
9	27.5	...	...	96	34.60	641.66	23.33	476.66	165.0	17.33	6
10*	27.5	...	Adverse.	...	27.80	641.66	23.33	476.66	165.0	17.33	6
11	34.5	...	Calm.	89	35.30	868.31	25.16	661.31	207.0	19.16	6
12	36.5	8	Favorable.	89	36.50	918.65	25.16	699.65	219.0	19.16	6
13	40.75	...	...	177	26.15	515.70	12.65	271.20	244.5	6.55	6
14	40.75	...	Side.	...	17.70	515.70	12.65	271.20	244.5	6.55	6
15†	40.75	...	Dead calm.	89	31.40	1025.61	25.16	781.11	244.5	19.16	6

* These two experiments were made on the same day, and the second as quickly after the first as the train could be returned to the summit.

† In this experiment the train had not quite acquired an uniform velocity.

These and other ingeniously devised experiments, made with the intent to discover the peculiar manner in which the air operates as a retarding force to

railway trains—but which do not affect the present analysis—led the author to adopt the conclusions given in the subjoined note *.

The fifth conclusion is one which I think the foregoing examination of M. de Pambour's experiments has already negatived, and we shall presently see additional reasons for doubting the authenticity of data which rate the resistance at a still higher sum.

The first part of the eleventh conclusion being founded on the accuracy of the fifth, the important inference thence deduced, must also be considered as negatived, if it be established that the resistance is less rather than greater than has hitherto been supposed.

* "1. That the resistance to a railway train, other things being the same, depends on the speed.

"2. That at the same speed the resistance will be in the ratio of the load, if the carriages remain unaltered.

"3. That if the number of carriages be increased, the resistance is increased, but not in so great a ratio as the load.

"4. That, therefore, the resistance does not, as has been hitherto supposed, bear an invariable ratio to the load, and ought not to be expressed at so much per ton.

"5. That the amount of the resistance of ordinary loads carried on railways, at the ordinary speeds, more especially of passenger trains, is very much greater than engineers have hitherto supposed.

"6. That a considerable, but not yet ascertained proportion of this resistance is due to the air.

"7. That the shape of the front or hind part of the train has no observable effect on the resistance.

"8. That the spaces between the carriages of the train have no observable effect on the resistance.

"9. That the train, with the same width of front, suffers increased resistance with the increased bulk or volume of the coaches.

"10. That mathematical formulæ deduced from the supposition that the resistance of railway trains consists of two parts, one proportional to the load but independent of the speed, and the other proportional to the square of the speed, have been applied to a limited number of experiments, and have given results in very near accordance; but that the experiments must be further multiplied and varied before a safe, exact, and general conclusion can be drawn.

"11. That the amount of resistance being so much greater than has been hitherto supposed, and the resistance produced by curves of a mile radius being inappreciable, railways laid down with gradients of about 16 feet a mile have practically but little disadvantage compared with a dead level; and that curves may be safely made with radii less than a mile; but that further experiments must be made to determine a safe minor limit for the radii of such curves, this principle being understood to be limited to its application to railways intended chiefly for rapid passenger traffic." (*Communicated by Dr. Lardner at the Meeting of the British Association for the Advancement of Science*, 1839.)

It appears from the tenth conclusion that the author considers his experiments, so far as they have gone, as giving results in very near accordance, and demonstrative that the resistance opposed by the air, increases as the square of the velocity. With the view of placing before the eye the degree of this accordance I have composed Table XII., which exhibits all the results fairly deducible from these experiments, excluding any comparisons between those made with a favourable, and others with an adverse, or side wind; comparing, in fact, those only which were affected by like circumstances as to weather. Nor have I compared the results arising out of the data applied to the experiments with different numbers of coaches, as, by the ninth conclusion, the bulk or volume of the mass in motion would appear to affect them. I must also remark that a different amount assumed for friction would alter the ratios of the resistance assigned to the air only, which would be increased or decreased as a higher, or lower rate for that amount were used. According to the manner of estimating the friction of coaches, 5 lbs. per ton would be assigned for some, and even 8 lbs. for others. It will be observed that in each set of comparisons the lowest speed is unity; and I have given the ratios of the direct, as well as of the squares of the velocities, that the eye may readily compare the results with them. The results are set down in the four last columns in the order of the ratios of the velocities, for the more ready appreciation of the degree of accordance which obtains among them.

It cannot fail to be remarked that the term discordance would seem to be much more appropriate than accordance to the indications of the last column of the Table, when referred to the ratios of the squares of the compared velocities in the last column but two. Nor does the mean of all these experiments appear to justify the conclusion that resistance to railway trains from the air is proportional to the square of the speed, for the mean of the 19 comparisons gives a much higher ratio for that resistance, viz. 2.92, the mean square of the velocities being as 1.99 to 1; but no fair average can be struck from such irregular results. The latter part of the author's sixth conclusion seems, therefore, to be more strictly consonant with them than the former part of the tenth.

Table XII.

Vide Table XI.		Ratio of the velocities.	Ratio of the squares of the velocities.	Ratio of the total resistance per ton.	Ratio of the resistance per ton from the air only.	Ratio of the velocities.	Ratio of the squares of the velocities.	Ratio of the total resistance per ton.	Ratio of the resistance per ton from the air only.
Comparison of Experim^ts.	1 with 2	1 to 1.08	1 to 1.16	1 to 1.00	1 to 1.00	1 to 1.07	1 to 1.16	1 to 1.00	1 to 1.00
	1 ... 3	1.46	2.15	1.84	2.60	1.08	1.16	1.00	1.00
	1 ... 4	1.36	1.85	1.84	2.60	1.08	1.17	1.03	1.05
	1 ... 5	1.22	1.50	1.07	1.10	1.08	1.17	1.07	1.10
	1 ... 6	1.63	2.66	2.76	7.07	1.11	1.23	1.49	2.71
	2 ... 3	1.58	2.51	1.84	2.60	1.13	1.28	1.08	1.10
	2 ... 4	1.47	2.16	1.84	2.60	1.16	1.35	1.07	1.10
	2 ... 5	1.13	1.28	1.08	1.10	1.19	1.44	1.49	2.71
	2 ... 6	1.76	3.10	2.76	7.07	1.22	1.50	1.07	1.10
	3 ... 4	1.07	1.16	1.00	1.00	1.36	1.85	1.84	2.60
	3 ... 5	1.80	3.24	1.99	2.88	1.39	1.94	2.46	4.56
	3 ... 6	1.11	1.23	1.49	2.71	1.46	2.15	1.84	2.60
	4 ... 5	1.67	2.79	1.99	2.88	1.47	2.16	1.84	2.60
	4 ... 6	1.19	1.44	1.49	2.71	1.58	2.51	1.84	2.60
	5 ... 6	2.00	4.00	2.97	7.82	1.63	2.66	2.76	7.07
	7 ... 10	1.16	1.35	1.07	1.10	1.67	2.79	1.99	2.88
Mean of 8 and	8 ... 9	1.08	1.17	1.07	1.10	1.76	3.10	2.76	7.07
	10 ... 11	1.08	1.17	1.03	1.05	1.80	3.24	1.99	2.8 8
	12 ... 13	1.39	1.94	2.46	4.56	2.00	4.00	2.97	7.82

Dr. Lardner developed, also, the following particulars of a very valuable experiment with an engine and train, from Liverpool to Birmingham, and back, conducted with much skill and precaution to obtain accuracy in the observation of velocity, and evaporation, by Mr. Edward Woods, engineer to the Liverpool and Manchester railway.

Experiment with the *Hecla* locomotive from Liverpool to Birmingham, and from Birmingham to Liverpool on the same day.

	Tons.
Weight of engine	12
——— tender	10
——— 12 coaches loaded with iron	60
Gross load	82
Useful load	60

Particulars of Engine.

	Inches.
Cylinders	$12\frac{1}{2}$ diameter.
Length of stroke	18
Wheels, 5 feet diameter.	

Consumption of coke and water.

	L. to B.	B. to L.	Mean.
Total, coke .....lbs.	3654	3406	3530
Coke per mile. .....lbs.	38.4	35.8	37.1
Water evaporated. .....cubic feet	337	300	318.5
——————— .....per mile	3.55	3.16	3.35
Coke per cub. foot of water .....lbs.	10.84	11.35	11.09
Force of water .....lbs.	2510	2234	2372

Total distance traversed 190 miles.

	ho.	min.	sec.
Time.—1st trip, including stoppages	4	28	58
2d Ditto, ditto	4	3	30
	8	32	28
Deduct for stoppages.....	2	5	40
	6	26	48
Supposing the 190 miles to be a dead level.....	6	8	54

	miles per hour.
Mean velocity during the trips	29.47
Difference for gradients	1.46
Mean velocity on a level	30.93

To reduce this experiment to the terms of the Tables on the author's data, it is necessary to assign a resistance overcome, and for that purpose to ascertain an inclination of plane, or "angle of repose," as he terms it, which would give an uniform velocity of 30.93 miles per hour to the mass. To this end, I have used the mean of experiments 8 and 10, and 11 in Table XI., which alone can be called unobjectionable. The last was made in a calm; the two first were performed as quickly as possible in succession on the same day, with the same coaches, the same wind being favourable to the one, and adverse to the other, so that their resulting mean gives a declivity, and a velocity equivalent to the production of a like effect in a calm, which is for the incline $\frac{1}{92.5}$, and for the speed 32.65 miles per hour. The mean of this last and of experiment 11, is for the incline $\frac{1}{90.75}$, and for the velocity 34.02 miles per hour, which, by inverse proportion, gives the inclination of $\frac{1}{100}$ for the "angle of repose" at a velocity of 30.93 miles per hour. Thus, the resistance or tractive force for the gross load of 82 tons is found, by the author's data, to be 1836.80 lbs. for the mass, or 22.40 lbs. for the ton. On comparing the resistance thus obtained,

with that assigned to nearly similar velocities in Table XI., the correspondence will be found very near; but it is below rather than above the value resulting from the author's method of deducing the resistance, for, according to his ninth conclusion, the bulk or volume of the mass augments the resistance, and the mass drawn by the *Hecla* is greater than that used in the three cited experiments with six coaches, in the ratio of 14 to 6.

The actual mean velocity over the ground in the two trips was 26.95 miles per hour, for which speed I have computed the "angle of repose," by the same process, to be $\frac{1}{116}$, the total resistance 1611.22 lbs. and 19.64 lbs. per ton.

For the purpose of separating the relative quantities of air resistance, and friction, I applied to Mr. Edward Woods, who performed the experiment, whose data for friction were used by Dr. Lardner, and who alone could inform me, from his practical knowledge, of its probable amount for the particular coaches used in this experiment. Mr. Woods states it as about 8 lbs. per ton for those 12 coaches, and I have assumed the same friction for the locomotive and tender; so that, for the velocity of 30.93 miles per hour the resistance of the air would be 14.40 lbs., and for 26.95 miles per hour, 11.64 lbs. per ton. The coaches were loaded with iron weights, and the two trips were performed on the same day, the one immediately succeeding the other.

Mr. Woods's minute and exact observations, during these two trips, will appear from his replies to my queries relative to the diminution of water consumed as steam requisite to be made for waste from delays on the road; an approximation to which necessary correction I could not have arrived at without being informed of the number, and duration of the delays. These are stated by him as follows:

Statement of the time occupied in the trips to and from Birmingham and Liverpool, with the *Hecla*.

	ho. min. sec.
1st Trip.—Liverpool to Birmingham, stoppages included	4 28 58

Time lost on the road.

	min. sec.	
1. Getting up speed at Liverpool	2 21	
2. Slackened over Parr Moss	3 17	
3. Stoppage at Warrington	34 48	
4. Ditto Hartford	5 34	
5. Ditto Crewe	7 12	
6. Slackened at Whitmore	1 34	
7. Stoppage at Stafford	8 29	
8. Ditto Wolverhampton	6 0	
9. Slackened at Birmingham	0 24	
		1 9 39
Time which would have been occupied if the train had started from Birmingham at full speed, and travelled from thence to Birmingham without stopping		3 19 19

	ho. min. sec.
2d Trip.—Birmingham to Liverpool, stoppages included	4 3 30

Time lost on the road.

	min. sec.	
1. Getting up speed at Birmingham	2 18	
2. Stoppage at Wolverhampton	5 11	
3. Ditto Stafford	5 52	
4. Ditto Whitmore	5 39	
5. Ditto Crewe	6 35	
6. Ditto Warrington	26 16	
7. Delay from overtaking train on the Liverpool and Manchester line	4 10	
		0 56 1
Time which would have been occupied if the train had started from Birmingham at full speed, and travelled from thence to Liverpool without stopping		3 7 29

Dead Stoppages.

First Trip.	min. sec.	Second Trip.	min. sec.
Warrington	32 34	Wolverhampton	3 13
Hartford	3 33	Stafford	2 25
Crewe	5 30	Whitmore	2 47
Stafford	6 35	Crewe	3 25
Wolverhampton	4 12	Warrington	24 15
	52 24		36 5

Deducting the dead stoppages, amounting to 88½ minutes, from the whole time, the actual period occupied in travelling was 7 hours, 3 minutes, 59 seconds. The total evaporation was 39812 lbs. effected in 512½ minutes; the proportion for the actual time of the trip, or 424 minutes, 32937 lbs.; difference 6875 lbs. The dead stoppages are divisible into two portions, viz. 9 very short ones comprising 31 minutes, 40 seconds; and 2 long ones equal to 56 minutes, 49 seconds. For the short stoppages I have deducted the full proportion, or 2465 lbs., as the steam would blow on the instant of stopping, perhaps even whilst the engine was coming to rest at the several stations, the regulator being closed some instants before arriving at them.

The two long stoppages at Warrington averaged each 28 minutes, 24½ seconds, but though, at the moment of stopping, the fire would be of the same intensity as during motion, it would be greatly diminished in force at the moment of starting again; for those two delays, therefore, a different correction is required as regards waste of steam. This I have endeavoured to supply.

Mr. Woods has informed me that the usual consumption of coke by engines waiting to start at the termini is about 100 lbs. per hour. This is an useful fact, as it determines, approximately, the difference in the rates of combustion and evaporation by locomotive boilers with the simple draught of the chimney, and with the acceleration given to it by the blast. Their difference on the *Hecla*, at the velocity of the journey, is about 8 to 1. If the evaporation for the whole delay at Warrington were reckoned as proportional to the time, with an active blast, it would amount to 4416 lbs.; and if computed according to the evaporative power of the boiler, without the blast, it would be only 552 lbs. Neither of these can be assumed as the real quantity. I have made a calculation from which I think the true amount of water vaporized in the 56 minutes, 49 seconds, would be very nearly approximated to in the sum of 2650 lbs.; but this may be thought too high, and I have concluded to reckon it as ⅓d of the active evaporation for the same time, or 1472 lbs., which is equivalent to burning 256 lbs. of coke in the time. Thus, the reduction for waste of water as steam during the delays is $2465 + 1472 = 3937$ lbs.

A further reduction is also necessary for such space as the engine may have passed over without steam in approaching stations, or in descending inclined planes, it being evident that steam would blow off in these intervals.

I found, on applying to Mr. Woods for an approximation with reference to these quantities, that 2500 yards may be deducted, on each trip, for that por-

tion of the ground over which the train passed, by virtue of its acquired impetus, when the steam was shut off whilst approaching the several stations; and that, on each trip, the train descended an inclined plane of a mile and a half in length by gravity alone.—These quantities amount to 30840 feet, and to 7852 cylinder volumes, or to between $\frac{1}{32}$d and $\frac{1}{33}$d part of the 190 miles traversed.

Mr. Woods also informed me, in reply to an enquiry on that head, that "there was a pretty constant simmering of steam from the safety valves during the whole journey." The amount of this portion of waste eludes determination, but, by its amount (which could not be inconsiderable during seven hours) the pressure per volume assigned in the Tables will be too high, as also the consumption of water as steam for the power. This constant waste through the safety valves proves the steam to have been constantly at the blowing off point, and that, when its exit through the cylinders was closed, it escaped freely; consequently, a proportionate deduction must be made for the spaces passed over without steam. By taking the time equivalent to the passage of the train over these 5.84 miles without steam, the quantity of water, ineffective as power from this cause, is found to be 1103 lbs. The effective evaporation, or the weight of water which actually passed through the cylinders, in the shape of steam, is, therefore, $39812-(3937+1103)=34772$ lbs., which quantity stands in the Tables as the consumption of the engine, and is exclusive of waste from the continuous escape of steam through the safety valves.

In estimating the pressure resulting from the comparative volumes of steam and water, $\frac{1}{20}$th is deducted from the water for the useless content of the cylinder passages, and $\frac{1}{32}$d from the volume of steam, for the space traversed without expenditure of power.

The consumption of coke is diminished in the same ratio with that of the water.

The experiment with the *Hecla* resolves itself into two cases:

1st. With reference to the consumption of water as steam, and of coke, applied to the actual mean velocity of the train.

2d. With reference to the consumption of water as steam, and of coke, applied to the velocity assumed by Dr. Lardner to be that which would have resulted, supposing the 190 miles traversed to have been a level plane.

I have computed and set down the results of each of these cases in the

Tables, but I have thought it unnecessary to fill up the columns for useful duty, and power, as the accuracy of the data of resistance is to be judged of from the whole tractive effort required, or gross duty done, compared with the power expended. I can assign no amount for engine friction, or blast-pressure, as the author does not profess to have made any experiments upon the engine to determine these quantities; consequently, no true value can be assigned for the pressure and power arising out of the sum of the resistances. These latter are entered in their respective columns, but they necessarily represent the values of the pressure and power equivalent to the tractive effect, or gross duty only, and have been computed for the sole purpose of comparing them with similar values from the volume test.

On turning to the Tables, and examining the results of this experiment, (case 2,) it will be apparent;

1. That a duty has been performed of double the amount effected by the condensing engine, with an equal expenditure of power. (Column 15.)
2. That, the absolute force impressed upon the pistons, as determined by the relative volumes of water and steam, was 30.95 lbs. per square inch; whereas the tractive effort, requisite to overcome the assigned resistance, amounted to 39.28 lbs. per square inch, exclusive of the force equivalent to the friction of the loaded engine, and blast-pressure. (Columns 29, 30.)
3. That, the power required of the engine to balance the tractive effort alone was 151½ horses; whilst the absolute power furnished by the steam to move the engine, to neutralize the blast-resistance, and to overcome the load, amounted only to 119½ horses. (Columns 33, 34.)
4. That, the water expended as steam per horse-power, per hour, was 37.89 lbs. for the tractive effect or duty only, (column 42); whereas, the condensing engine consumes 70 lbs. per effective horse-power; and, if the two engines were brought to an equality of comparison, the water expended by the locomotive per effective horse-power, per hour, must have been considerably less than 37.89 lbs., as the steam overcame the blast-resistance, in addition to its load.
5. That, compared with a fixed non-condensing engine, at equal pressure, the locomotive—though labouring against the heavy counter pressure of the blast, from which the other is free—is assumed to have performed equal work, with less than one-half the expenditure of power.

6. That, if the resistance assigned by Dr. Lardner as opposed to the progressive motion of the train be correct, the efficiency of the steam in the locomotive, is more than double that obtained by the best condensing engines; more than treble that derived from stationary non-condensing engines; and equal to the performance of a Cornish expansive engine doing a 50 million duty with a bushel of coals.

Such are the incredible results arising out of data, purporting to be fairly and necessarily deduced from unimpeachable experiments. To obtain credence for such results, which, until proved, can be deemed but hypotheses, it is incumbent on those who promulgate and receive them, to explain the process by which the locomotive, commonly considered as the most prodigal of engines, transmutes its apparently enormous consumption of steam into an economy so striking as to shake, if true, all reliance on the fixed and well founded principles by which engineers have hitherto been governed in the application of the steam engine to its various uses.—Until these anomalous and mysterious properties of the locomotive, in its use of steam, be brought to light, and satisfactorily accounted for; or, until a much nearer accordance be established between these hypothetical effects and the ascertained performance of other engines, the resistances assigned as opposed to and overcome by the locomotive, at different velocities, must be regarded by persons conversant with the power of steam, and with the practical capability of engines, as utterly inconsistent with reality, and as resting on no solid foundation. The fallacy of all these data is not only demonstrable by argument, but it is demonstrated by facts furnished by the engine itself, whenever an evaporation approximate even to the truth can be arrived at.

It is important to point out one other error into which railway analysts have fallen, and which is clearly exemplified by the separation of the experiment with the *Hecla* into two cases. I allude to the theoretical methods adopted by them for reducing undulating surfaces to a level.

M. de Pambour extends the length of the Liverpool and Manchester railway from $29\frac{1}{2}$ to $34\frac{1}{2}$ miles, as a compensation for the acclivities, or for the help afforded to his trains in surmounting them by the bank engines, which is the same thing. Dr. Lardner, on the contrary, reduces the actual time of the journey, to that which he assumes would be occupied in performing it on a dead level; this is equivalent to a certain increase of velocity which he assigns, and he supposes the same load to be moved at that increased velocity, with the same power, as at the lower velocity, but he assigns a very different amount of

resistance for each velocity. Now, there is one indubitable measure of the accuracy, or fallacy of these substitutions, viz., the effect produced by the power expended.—In the two cases of the *Hecla* an equal quantity of water as steam has been consumed by the engine, in dragging the same train over the same length of ground; consequently, the power ought to have produced an equal mechanical effect in both cases. How stand the results? By column 29, it is seen that an equal absolute pressure of steam existed on the pistons in both cases, and an equal number of cylinders have been filled with steam and discharged in passing over it; but the time in which the steam was expended is less by 14 per cent. in the 2d case than in the 1st: it is, therefore, certain, that the resulting pressure of the steam upon the pistons could not have been identical in the two cases.

Again, if the resistances assigned at the two velocities were relatively correct—however positively erroneous they might be—the duty done by the same power would be alike in both cases; but this is so far from being the presumed fact, that 12 per cent. more duty has been accomplished at the higher than at the lower velocity. (Column 15.)

The same discrepancy appears on referring to the amounts of absolute and tractive power of the engine; a power, let it be borne in mind, created by an equal expenditure of steam in the two cases. Column 33 shews the difference between the absolute powers to amount to $15\frac{1}{4}$ horses; and column 34 exhibits the same engine, using the same quantity of steam, to have exerted in the 1st case an effort equal to $115\frac{1}{2}$, and in the 2d case, an effort equal to $151\frac{1}{2}$ horses power, or a difference of 36 horses power. No change whatever, be it observed, is supposed to have taken place in the sum total of the forces opposed to the engine, for the same absolute pressure is assumed to have urged the pistons; the friction and blast resistance in the 2d case, (that of the level,) must, therefore, have been nearly if not precisely the same as in the first case; and the conclusion is inevitable that, if the resistance assigned for the load at 26.95 miles per hour be supposed correct, the amount attributed to it at 30.93 miles per hour is very greatly exaggerated; or we must conclude that the method resorted to for reducing the undulating to a level plane is erroneous.

There are numerous practical difficulties against reducing a railway to an imaginary level so as to make the mechanical effects of a given power, on the level, coincide with those obtained under the practical conditions of a railway; and it is manifest that these results ought to agree, if every circumstance con-

nected with the problem be taken into account. If all the resistances and frictions incidental to the engine and its train were truly ascertained for all velocities, and all loads, and corrections were applied accordingly, the effects obtained from a given power moving the same load, at two different velocities, over a level, and over an undulating surface, would be identical. The comparison of these two cases shews the data of resistance to be altogether fallacious; and I think the effects ascribed to a locomotive engine of the dimensions of the *Hecla* will be regarded as fabulous, when it is seen that, in order to accomplish a tractive effort exceeding 150 horses power, the whole force of steam upon the pistons could not have amounted to less than 200 horses; for it cannot be supposed that the friction of the engine so highly loaded, together with the blast-pressure, required less than 12 lbs. per square inch to balance them; which addition would raise the absolute power of the engine to 200 horses. It is, I apprehend, very questionable whether the parts of any locomotive engine yet constructed could hold together, under a total effort of even half that power.

By these observations I am far from presuming to dispute the accuracy of the compensations applied for the acclivities and declivities of the railways traversed in this experiment, as reckoned on the time; but, if the principle on which the level has been calculated be true, the comparison of the two cases establishes the necessity of appreciating other facts than we are at present acquainted with, before it can be demonstrated that a given power will convey a given load at some certain increased velocity along a level, compared with the actual velocity along any given undulating line. This is a practical question of very great import to the science, as well as to the first cost, and ultimate mercantile success of railways, and it is of the utmost consequence that its determination should be sought in a practical sense, and by such practical methods as railways and engines enable us to use. Such a problem as this is not to be solved by deductions from experiments on wagons and carriages running down inclined planes; facts alone can decide it, and they are attainable.

There is no such thing as a constant quantity, in a practical point of view, appertaining to any one of the resistances or frictions which enter into the composition of the sum of the forces opposed to the progressive motion of an engine and train. There can be no doubt but that the air opposes different amounts of resistance to the same body moving through it with different velocities, so that in ascending an acclivity, though the velocity of a train be diminished by the counter force of gravity, the resistance to the train from the air is di-

minished at the same time; and a correction would be required for this quantity at every, even the smallest, deviation of the line from a mathematical level, whether ascending or descending. The pressure of steam upon the pistons varies with the effort required, causing fluctuations in the amount of engine friction and blast-resistances according to the velocity. In the cases under consideration a level has been calculated, and it may be on sound principles, but it does not appear that any corrections have been applied for these varying forces as regards the load, or power; consequently, the theoretical level is a pure fiction with reference to the practical results of the experiment.

The same remarks apply to M. de Pambour's experiments reduced to his level.

I should state, with reference to the amounts of water consumed as steam set down in column 11, for the two cases of the *Hecla*, that the difference arises from having to treat a hypothetical case, viz. that of a level. In the 1st, or actual case, two inclined planes of $1\frac{1}{2}$ mile each occurred, which the engine traversed without steam, besides the 5000 yards in approaching stations, by which the consumption of water had to be diminished $\frac{1}{375}$d, as previously explained. The imaginary case of the level requires that the engine should be supplied with steam along the 3 miles deducted for these inclined planes in the 1st case; consequently $\frac{1}{67}$th is the correction in the 2d case for the 5000 yards only. A similar correction for the space passed over without steam was requisite in estimating the volume of steam consumed; as the power was in action in the 1st case, during 61894 double strokes of each piston, and in the 2d case during 62902 double strokes. The mean velocity of the engine and train in both cases is found by dividing the whole distance traversed, by the whole time; but, as the steam did not operate during the whole time in either case, a further correction became necessary for the number of revolutions of the working wheel, or number of double strokes of the piston effected per minute by the steam. In the 1st case the steam operated during 411 minutes instead of the whole time, or 424 minutes; and in the 2d case during 363.4 minutes, instead of 368.9 minutes. Had not these corrections been made, the number of horses power arising out of the assigned resistance, (column 34,) would not have corresponded with the power deduced from the duty, (column 36); and it is evident that the two, being identical, ought to coincide in amount, as they do when they are properly computed.

TABLE of the volume of the steam generated under different pressures, compared to the volume of the water that has produced it.—*New Theory of the Steam Engine by Le Comte de Pambour, page* 76.

Total pressure, in English pounds, per square inch.	Corresponding temperature by Fahrenheit's thermometer.	Volume of the steam compared to the volume of the water that has produced it.	Total pressure in English pounds, per square inch.	Corresponding temperature by Fahrenheit's thermometer.	Volume of the steam compared to the volume of the water that has produced it.	Total pressure, in English pounds, per square inch.	Corresponding temperature by Fahrenheit's thermometer.	Volume of the steam compared to the volume of the water that has produced it.
1	102.9	20954	38	265.3	710	75	308.9	381
2	126.1	10907	39	266.9	693	76	309.9	377
3	141.0	7455	40	268.4	677	77	310.8	372
4	152.3	5695	41	269.9	662	78	311.7	368
5	161.4	4624	42	271.4	647	79	312.6	364
6	169.2	3901	43	272.9	634	80	313.5	359
7	176.0	3380	44	274.3	620	81	314.3	355
8	182.0	2985	45	275.7	608	82	315.2	351
9	187.4	2676	46	277.1	596	83	316.1	348
10	192.4	2427	47	278.4	584	84	316.9	344
11	197.0	2222	48	279.7	573	85	317.8	340
12	201.3	2050	49	281.0	562	86	318.6	337
13	205.3	1903	50	282.3	552	87	319.4	333
14	209.0	1777	51	283.6	542	88	320.3	330
15	213.0	1669	52	284.8	532	89	321.1	326
16	216.4	1572	53	286.0	523	90	321.9	323
17	219.6	1487	54	287.2	514	91	322.7	320
18	222.6	1410	55	288.4	506	92	323.5	317
19	225.6	1342	56	289.6	498	93	324.3	313
20	228.3	1280	57	290.7	490	94	325.0	310
21	231.0	1224	58	291.9	482	95	325.8	307
22	233.6	1172	59	293.0	474	96	326.6	305
23	236.1	1125	60	294.1	467	97	327.3	302
24	238.4	1082	61	294.9	460	98	328.1	299
25	240.7	1042	62	295.9	453	99	328.8	296
26	243.0	1005	63	297.0	447	100	329.6	293
27	245.1	971	64	298.1	440	105	333.2	281
28	247.2	939	65	299.1	434	120	343.3	249
29	249.2	909	66	300.1	428	135	352.4	224
30	251.2	882	67	301.2	422	150	360.8	203
31	253.1	855	68	302.2	417	165	368.5	187
32	255.0	831	69	303.2	411	180	375.6	173
33	256.8	808	70	304.2	406	195	382.3	161
34	258.6	786	71	305.1	401	210	388.6	150
35	260.3	765	72	306.1	396	225	394.6	141
36	262.0	746	73	307.1	391	240	400.2	133
37	263.7	727	74	308.0	386			

Table X.

No. of experiments.	Total horse power deduced from the pressure per volume of steam.	Horse power deduced from the pressure of the sum of the resistances.	Effective horse power.	Tractive horse power deduced from the gross duty.	Useful horse power deduced from the useful duty.	Horse power arising from the difference of pressure on the pistons, deduced from the volume of steam, compared with that from the sum of the resistances.	Water consumed per hour.	Coke consumed per hour.	Consumption of water as steam per horse power per hour.					Consumption of coke per horse power per hour.				
									Per horse power from the volume of steam.	Per horse power from the sum of the resistances.	Per effective horse power.	Per tractive horse power.	Per useful horse power.	Per horse power from the volume of steam.	Per horse power from the sum of the resistances.	Per effective horse power.	Per tractive horse power.	Per useful horse power.
	H. P.	H. P.	H. P.	H. P.	H. P.	H. P.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
I.	48.93	49.41	.....	42.92	39.41	− 0.48	2723.07	526.15	55.65	55 11	.....	63.44	69.09	10.75	10. 6	.....	12.25	13.35
II.	45.62	52.07	.....	44.65	39.24	− 6.45	3018.81	622.37	66.17	57.97	.....	67.61	76.93	13.64	11.95	.....	13.93	15.86
III.	37.06	25.00	.....	20 16	13.61	+12.06	2907.36	463.68	78.45	116.02	.....	144.21	213.61	12.51	18.54	.....	23.00	34.07
IV.	32.54	27.35	.....	21.66	12.99	+ 5.19	3180.00	502.32	97.92	116.27	.....	146.81	244.80	15.43	18.36	.....	23.19	38.67
V.	42.52	40.80	.....	28.22	18.90	+ 1.72	3783.18	709.16	88.97	92.72	.....	134.06	199.21	16.67	17.38	.....	25.12	37.34
VI.	46.18	34.80	.....	27.19	19.53	+11.38	3620.25	517.40	78.39	104.03	.....	133.14	180.24	11.20	14.86	.....	19.02	26.49
VII.	62.67	44.73	.....	37.61	33.12	+17.94	3782.52	566.52	60.35	84.56	.....	100.57	114.20	9.03	12.66	.....	15.06	17.10
VIII.	58.72	31.31	.....	25.11	18.23	+27.41	4116.38	534.19	70.10	131.47	.....	163.93	225.80	14.53	17.06	.....	21.27	29.29
IX.	49.77	31.73	.....	26.49	20.36	+18.04	3439.57	471.18	69.10	108.40	.....	129.84	168.93	9.46	14.85	.....	17.78	23.14
X.	43.33	37.57	.....	31.57	25.10	+ 5.76	3252.00	537.33	75.05	86.55	.....	103.00	129.56	12.40	14.30	.....	17.02	21.40
XI.	88.66	76.64	.....	65.70	61.15	+13.03	4812.00	616.00	53.60	62.78	.....	73.19	78.07	6.86	8.03	.....	9.37	10.73
XII.	44.73	40.30	.....	29.83	18.46	+ 4.43	4812.00	616.00	107.35	119.40	.....	161.17	260.69	13.77	15.28	.....	20.65	33.36
XIII.	67.24	67.11	49.65	42.41	39.37	+ 0.13	3026.00	.....	45.00	45.09	60.00	71.35	76.89	.....	.....	.....	.....	.....
XIV.	53.55	59.50	35.42	13.73	12.87	− 5.95	3026.00	.....	56.50	50.85	85.43	220.39	235.12	.....	.....	.....	.....	.....
XV. 1	104.29	115.51	.....	115.52	.....	−11.23	4920.56	858.11	47.18	42.59	.....	42.59	.....	8.22	7.42	.....	7.42	.....
XV. 2	119.52	151.68	.....	151.36	.....	−32.16	5747.89	1002.54	48.08	37.89	.....	37.97	.....	8.38	6.60	.....	6.62	.....
	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50

OF MOMENTUM AS A MEASURE OF THE EFFECT OF LOCOMOTIVE ENGINES.

The effective power of a locomotive engine—by which is meant the excess of power after overcoming its proper friction, and the resistance from the blast—is solely expended in the generation of momentum. The momentum communicated to the entire mass set in motion represents the useful mechanical effort exerted by the steam; this effect of the engine is, therefore, at all times determinable; for, being the simple product of the mass moved, multiplied into its velocity, it is the product of two quantities easily ascertained under all the practical circumstances of railway traffic. The consumption of power, as water in the shape of steam, is a third quantity also readily appreciable.

Were it possible to work a locomotive engine and its train *in vacuo*, on a truly level plane, the momentum generated by an equal expenditure of power would be a constant quantity at all velocities; for, the resistance being invariable, equal momenta would be produced by an equal expenditure of power with all loads, as the velocity attained would be in the inverse ratio of the loads, and vice versâ. This hypothetical case supposes friction and resistance of all kinds to be constant. We know, however, in practice, the power and velocity of an engine and train to be limited by the interference of numerous impediments, which may be enumerated as follows:

1st. The air—the amount of resistance or assistance from which source, must be nearly as inconstant as the wind itself—has to be displaced; a passage has to be forced through it; but, in what ratio this obstacle varies with the velocity of a train, or in what proportion it enters into the total sum of the resistance to be overcome, at any one velocity, we are at present utterly ignorant.

2d. The friction of a body moving upon an inflexible uniform plane is commonly assumed to be the same, or nearly so, at all velocities for equal weights, but it is questionable whether this assumption be well founded in its application to railways, as they are very far from being inflexible uniform planes, but consist, on the contrary, of a series of short elastic bars, very imperfectly united, with points of union which offer sensible obstructions to a train passing over them. No determinate experiments have

yet acquainted us with the value of this friction at any one velocity; nor do we know whether it increases or diminishes, or in what ratio with the speed and weight of a train. The practical condition of a railway is also at no time so perfect as to present, for many yards successively, either a true rectilinear or a true transverse level; circumstances which—inasmuch as they vary greatly in different parts of the same railway, and still more in different railways—must affect the amount of friction.

3d. The blast-pressure—which is actually, though not intrinsically, a condition of the engine—opposes to the power amounts of resistance fluctuating with the load, and with the velocity. Its intensity may be supposed to have some definite relation to that of the active steam, and to the number of discharges from the cylinders which have to be made in a given time; but its value is, at present, unascertained for any one load, at any one velocity.

4th. The friction of the moving parts will be a constant quantity for the same engine, without load, and in good order; but it necessarily varies for engines of different dimensions and weights, and according to the method of applying the power, number and diameter of the wheels, &c.

5th. The additional friction brought upon the moving parts of engines by their load, and by the velocity at which they are travelling, is also a fluctuating quantity, and one which it is extremely difficult to ascertain.

Such is the inconstant nature of the principal forces which have to be overcome by the power applied to a locomotive engine. All attempts to determine their special as well as their total value have proved fruitless, and the widely discordant estimations assigned by the experimenters whose labours have been examined, exhibit the difficulties of arriving at the true determination of some of these forces, to be undiminished, if not insurmountable.

Until the value of the blast-pressure be definitely ascertained, no true estimate of the effective power of an engine employing it can be made; consequently, the force of such engine cannot be measured by the consumption of water as steam; for, all the power employed to overcome the resistance of the blast is ineffective. Until, therefore, this particular resistance be determined for every pressure on the piston, no quantity of water as steam can be correctly assigned as the measure of an effective horse-power in a locomotive; nor can we accurately compare its effects with those of other engines. All we do know is that, by that amount, the locomotive must expend more

steam for equal effects, than a fixed non-condensing engine, at equal pressures, if their friction be the same.

A knowledge of the momentum generated by an equal expenditure of water as steam in equal times, furnishes a true test of the useful mechanical performance of the engine, as well as a true test of the difference of power consumed in the production of equal momenta, or of the same useful mechanical performance at different velocities; for, the expenditure of steam varies only according to the absolute resistance opposed to it, which it exactly balances, and, therefore, accurately measures. The momentum of the mass moved being a measurable product of the power, and this product diminishing with every increase of velocity, in the same ratio that the increments of resistance opposed to the acquisition of such higher velocity augment, the quantity of power employed to generate equal amounts of momentum becomes a true measure of the increased resistance overcome by that power, from whatever sources it may arise. These principles are embraced in the two following propositions:

Proposition 1.—Equal momenta would result at all velocities from an equal amount of power expended in equal times by the same engine, if the forces opposed to progressive motion, and to the effective use of the steam in the engine, were uniform at all velocities.

Proposition 2.—The difference between the momenta generated by an unit of power in a given time at various velocities, measures the difference in the sum of the resistances opposed to the power at those velocities.

By ascertaining, therefore, three facts which are at all times within our reach, viz. the gross weight of an engine, tender and train—their mean velocity—and the expenditure of water as steam during the trip, very simple computations inform us;

1st. Of the mechanical effect realized by a given power at all velocities.

2d. Of the total increase or decrease of resistance at all velocities.

3d. Of the ratios which the increase or decrease of resistance, at different velocities, bear to the ratios of those velocities.

Two other results, distinct in their nature from the foregoing, are disclosed by a knowledge of the same three facts, viz. the amount of gross and useful tractive effect realized by an equal expenditure of power at all velocities. These form what may properly be termed the commercial results, of which the latter is the only useful quantity. The difference between these two

amounts is an useless quantity in a practical sense, and is a costly waste of power incident to the locomotive functions of the engine and tender, over and above the waste arising from the unascertained and ineffective portion of the whole power required for blast*.

Tables XIII. and XIV. contain the reductions of a large assemblage of results to the terms necessary for the exhibition, and development of these views. They comprise forty-nine experiments, being all those previously referred to, with the addition of the experiments by Mr. Nicholas Wood on the Great Western and London and Birmingham railways, and others. To these experiments I have added (Table XIII.) the dimensions of the engines, that particulars, so necessary for every investigation into the performance of the locomotive, may not be wanting for any one engine referred to in this paper.

Table XIII. registers the velocity of the engines, the consumption of water as steam, the loads, the absolute momenta per second, the momenta generated by equal power in equal times, viz. by 1 lb. of steam per second—also the weights of the gross and useful loads moved by equal power, viz. by 1 cubic foot of water as steam, at the velocity of each experiment. The water stated in columns 4 and 12 is the whole quantity expended during the trips, inclusive of all losses which may have taken place, excepting when corrections have been supplied, as in the case of the *Leeds* (*new data*,) and *Hecla*. The water assigned as the consumption of the *Atlas*, experiment III., is computed proportionally to the other experiments on that engine, M. de Pambour not having noted it, and it being important to have the means of comparing two pairs of results with this engine, on the actual and assumed distances of 29½ and 34½ miles, separately, as false conclusions only could arise out of a comparison of the real with the fictitious distance traversed†.

* These two losses constitute the principal difference in the economy of power between stationary and locomotive engines, as instruments of railway traction; the former having to suffer a deduction for loss of power arising from the use of the rope. The precise relation of the two engines in the production of useful effect by an equal expenditure of power, offers many interesting points for investigation, which have latterly been little enquired into. Many important facts connected with railway science, and with the profitable adaptation of railways to other than rapid passenger traffic, require to be, and could not fail to be illustrated by a well arranged, definite series of experiments on both engines.

† For the comparative purposes of these tables, the actual evaporation from the boilers can only be used as data, unless every one of the experiments had been supplied with accurate corrections for waste. As they stand in these tables, we have the quantities of water practically expended as power, with which it is necessary to rest contented until experiments be conducted

Column 11 exhibits the momentum, or product of the mass, in tons, of the engine, tender, and train multiplied into its velocity in feet per second; and the sums thus represent the respective mechanical effect produced per second by each engine. If precisely equal quantities of water as steam had been expended in equal times, the sums in this column would have accurately shewn the relative effects realized by the expenditure of an equal force at the different velocities, and with the different loads; but the consumption of a given power, in a given time, formed no condition of the experiments, nor could it be practically accomplished. In order, therefore, to obtain a true expression of the effect in terms of power, it is necessary to find the momentum proportionate to each unit of power expended in its generation. The unit chosen is one pound of water as steam. These results are accordingly given in column 12, and the effect of a pound of water as steam—or a pound of power—at any one of the velocities recorded in column 3, is strictly comparable with a like effect, at any other velocity, by the notation adopted in column 12.

The series of results in this last column serves, also, to a certain extent, as a test of accuracy in the conduct of the experiments, for we know that the same mechanical effect cannot be realized at a high, as at a low velocity, by the same engine; we know that an engine dragging a load at 23 miles per hour, must consume more power, to acquire an equal momentum, than at a lower velocity. A reference to the *Fury* (previously adverted to as giving anomalous results,) exhibits that engine as having performed more work at 23, than at $21\frac{3}{4}$ miles per hour, by the ratio of 24 to 19; it is, therefore, with certainty

with such care and precision as to require no corrections, or such only as are, in their nature, unavoidable. These quantities fairly represent, perhaps, the comparative consumption of power for given effects on the different railways, as we cannot but suppose the engines, when under charge of the parties who conducted these experiments, to have been managed with greater care than when left to the sole guidance of the attendant engineer, as in common practice. Mr. Wood has assigned no value for loss of steam by the safety valves, delays, or other sources, so that any attempt to determine the positive power of those engines would be fruitless; and if M. de Pambour's data for water were corrected, and the others uncorrected, the comparison of effects resulting from an equal expenditure of power by the different engines, on the different railways, would be entitled to no credit. If the evaporation in M. de Pambour's experiments were diminished by one-fourth, according to his instructions, or by any other equal amount, the relative comparisons between his engines would still hold good; but it is evident that the relation between his results and others would be altogether faithless, unless it were certain that the latter required no correction, or unless an ascertained correction were supplied.

we may conclude one or both of those experiments to be erroneous, though we cannot determine which of them, for want of a sufficient series of results from the same engine to guide our judgment. For subsequent comparison, therefore, I have struck out these two experiments as unsatisfactory.

The *North Star* affords a sequence of six experiments at velocities varying from 18½ to 38½ miles per hour, but the sequence of results is so irregular as to indicate error in two of them, which I have accordingly marked and rejected, for it is certain that a greater measurable effect must accrue from the expenditure of equal power at 25 than at 30, and at 31½ than at 34 miles per hour; yet, the reverse appears on the face of the experiments. It is also equally impossible that a greater momentum should have been generated by a like consumption of force at 34, than at 25 miles per hour.

The seven experiments on the *Æolus* present a pretty uniformly descending gradation in the momenta resulting at speeds between 17¾ and 33¼ miles per hour, excepting Experiment II., which must be discarded as erroneous.

Evidence of accuracy is satisfactorily developed by other trials, the selection of one or two examples of which will serve to illustrate, and direct attention to the value, and searching nature of this test. Experiments I. and II. on the *Neptune*, at velocities almost identical, but with a load somewhat greater in the second than in the first, shew an accordance in their results so close as to merit confidence in the conduct of them. The two experiments on the *Lion* are even more conclusive, the velocities being nearly identical, yet it is seen that power was consumed in exact proportion to the loads, though they varied by 25 per cent.; and, consequently, that the same measurable effect, or equal momenta, resulted from the consumption of equal power at similar velocities. These experiments exhibit the great importance of a repetition of trials at like velocities with like, or even unlike loads, to inspire perfect confidence in their results. The four experiments on the *Apollo*, as also those on the *Harvey Combe*, and *Bury's engine*, afford rational grounds for considering them to be respectively entitled to credit, inasmuch as the descending scale of their effects comports with the ascending scale of their velocities. The same remark applies to M. de Pambour's series, with the exception of the *Fury*.

M. de Pambour has noted the state of the atmosphere during his experiments, none of which appear to have been affected by any particular force of wind either favourable, adverse, or transverse. I find no mention made by Mr. Wood of any extraordinary derangement from this source, but it must be

understood, as before remarked, in citing these experiments as worthy of confidence, that expression extends only to the mutual comparison of their results, and involves the supposition that the correction for loss of steam from waste, delays, &c., would be pretty much the same, for the same engine, in each experiment.

Table XIV. is a summary of the ratios of the velocities, and of their squares, brought into juxta-position with the ratios of the power expended to produce equal momenta, equal gross, and equal useful effects, by the comparison of pairs of experiments on the same engine given in Table XIII., and referred to in column 2. The actual velocities of each contrasted pair are reproduced in column 3, it being important to observe, not only the ratio of the velocities, but the particular speed of the compared engines. The lowest velocity of each pair is considered as unity. The influence which velocity exerts over the expenditure of power for equal mechanical, and equal commercial effects, is clearly manifested upon each engine in columns 6, 7, 8; and the amount of loss, attributable to the increase of resistance at the higher velocities, is shewn in the last three columns.

Four Means are derived from these results.

Mean 1. as explained in the table, embraces the whole number of experiments entitled to confidence in Table XIII., and this large average—which the results of 42 experiments upon 15 different engines may be considered—informs us;

1st. That when velocity is increased in the ratio of 1.52 to 1, an increased consumption of power is required for the production of equal mechanical effects, or of equal momenta, in the ratio of 1.43 to 1, being somewhat less than in the direct ratio of the velocities.

2d. That power is expended in the ratio of 2.43 to 1, or in about that of the squares of the velocities to produce equal gross commercial results.

3d. That power is expended in the ratio of 3.11 to 1, or in not much less than that of the cubes of the velocities, to realize equal useful commercial results.

In other words, it appears, from the mean of all these experiments, that a loss of 68, 59, and 30 per cent. on the useful, gross, and mechanical effects, respectively, arises from an equal expenditure of power at a velocity of 30.92 miles per hour, compared with a velocity of 20.32 miles per hour. It is important to have an exact method of arriving at such general results as these, but it is of still

greater consequence to be able to test, and ascertain the relative qualifications of particular engines, and the relative perfection of different railways; to the latter purpose I will first apply the powers of this method of analysis.

The Means 2, 3, 4, afford special results from these experiments on three railways, which distinctly mark the order of economy to be, 1st. Liverpool and Manchester; 2d. London and Birmingham; 3d. Great Western; as developed by the realization of the greatest mechanical effect, and of the greatest useful commercial effect, from an equal expenditure of steam power. It is, however, incumbent upon me to guard against these conclusions being considered as other than the indications of results flowing from the particular experiments which are here collected together; experiments which can neither be considered so numerous, nor so exact, as definitive determinations of so important a nature require. The Liverpool and Manchester railway has undergone certain changes in the weight of its rails since M. de Pambour's experiments; the engines may also be of other dimensions, and, for aught I know, more or less economical of power. Two engines only were subjected to trial by Mr. Wood on the London and Birmingham railway, and only over a small part of it. The Great Western railway was incomplete, the engines of a rather novel construction, and, at the time, somewhat recently put to work. It is, therefore, to be understood that a method of investigating the economic qualities of locomotive engines, and of railways generally, is here alone attempted to be illustrated, not the actual state of perfection of any one engine, or of any one railway, for which purposes I can only, at present, use the materials furnished to my hand.

The Means in Table XIV. are insufficient for either of these objects, as a true comparison of the qualities of engines requires that their effects should be manifested, as nearly as possible, at similar velocities; for, we are ignorant whether the resistances increase by the same ratios at low, as at high velocities: careful and repeated practical experiments can alone determine whether the rate in which resistance augments be the same, for example, between 10 and 20, as between 20 and 40 miles per hour.

For the more perfect exhibition of this method of analysis, and for the most comprehensive development of its powers afforded by the recorded experiments, Table XV. has been composed, which contrasts the results of six experiments on the Great Western, with six on the London and Birmingham railways, at velocities as nearly as possible identical. The power of the engines, at the mean speed of 27 miles per hour is represented in column 2, by the gross loads

respectively dragged at that speed; and it appears from the ratios of the loads, that the London and Birmingham engines were less powerful than those of the Great Western by 24 per cent.; but, by column 3, we learn that though the Great Western engines were more powerful—as they ought to be from their dimensions—the London and Birmingham engines performed 18 per cent. more work with an equal consumption of power.

The weight of useful load dragged is also in favour of the Great Western by 19 per cent., (column 4,) but column 5 shews that useful load to be accomplished by a considerable sacrifice of power, and, therefore, by a greater expense than an equal useful load on the London and Birmingham; the difference being 27 per cent. in favour of the latter railway. These are the commercial results incontestibly indicated by the comparison of the loads, and consumption of power assigned to the engines; but, before proceeding further in the investigation, I may observe that these results do not necessarily prove the system adopted on the one railway to be more or less economical than the other; as in a question of such magnitude, other important considerations and details have to be taken into account. It may happen to be a matter of extreme consequence to the interests of one railway to have a power at command capable of conveying a much larger number of passengers at one time, than another railway, and though this object may not be effected without a sacrifice of power by an individual engine, it may suit the necessities of that railway, and be cheaper than employing more engines, and more trains. I know not whether of the two, the Great Western or London and Birmingham, require to drag the heavier net load—a fact apart from this investigation—but we see that in order to gain an increased amount of useful load of 19 per cent., 27 per cent. more power was absorbed by the Great Western's engines.

The true comparison of the effective performance of the engines on the two railways is exhibited in column 6, by which it appears that the mechanical effect of the *Harvey Combe* and *Bury's engine*, was greater, from an equal expenditure of power, by 18 per cent., than that obtained from the engines compared with them.

Supposing a fact of this nature to be established by experiments of unexceptionable authenticity, the question arises as to its cause or causes, a question seriously involving the interests of existing, and of future railways. The enquirer, after satisfying himself that the engines were equally well constructed, and the power equally well applied, would naturally and necessarily be led to

examine the difference in the gauges of the rails, and in the weight of matter uselessly put in motion on the compared railways; an increase in the latter being an effect consequent to the adoption of a wider gauge, and the width of gauge itself influencing the resistance to progressive motion. I have given in column 7 the mean weights of the engines and tenders under comparison, and in column 8 the widths of the gauge of the two railways. The ratios of these are also given, as for the other facts, considering the Great Western to be unity, and the comparison shews that, with a gauge 34 per cent. less in width, the two London and Birmingham engines realized an equal mechanical effect with 18 per cent. less consumption of power: it is also seen that they realized 27 per cent. greater useful effect; and, by turning to column 7, it appears that they had 38 per cent. less useless load to drag, on that portion of the gross load composed of the weight of engine and tender. The conclusion, therefore, would be —on the supposition that no doubt existed as to the correctness of the experiments, and that they were sufficiently numerous, and sufficiently varied—that the difference in effect was produced by the conjoint operation of the width of gauge, and increased weight of useless mass.

Supposing these facts to be indisputable, or to be granted for the sake of argument, the particular values due to the increased resistance, and increased useless load, as separate quantities, are discoverable and assignable. Assuming, for this purpose, the highest results (those obtained on the London and Birmingham) to be unity, it appears that by increasing the width of gauge one half, or 50 per cent., the weight of engines and tenders—or the useless mass—is increased 59 per cent. on the Great Western. The acquisition of momentum, or of measureable mechanical effect, would, in no degree, be influenced by this change of weight; but, it is clear that some cause or causes operated to diminish this effect, for column 6 shews that the Great Western engines, with an equal expenditure of power, realized 16 per cent. less momentum—or, what is the same thing, had to overcome 16 per cent. greater resistance—than the engines on the London and Birmingham, at equal velocities. Now, it appears by Mr. Wood's Report that, of the portion of the two railways respectively traversed by the engines, the Great Western approached the nearest to a level; and, consequently, that the additional resistance encountered by the Great Western's engines, must spring from some other, and some very influential source. It would, therefore, from the facts before us—and on the assumption that all the engines compared were, mechanically speaking, equally perfect—

seem to be a fair conclusion that a width of gauge increased by 50 per cent. over another width, augments resistance to progressive motion by 16 per cent.; and that by reason of such additional resistance, and of the additional useless load caused by the width of gauge, the commercial or useful tractive effect, for equal power, is diminished by 22 per cent.; a conclusion which attributes 16 per cent. as loss arising from increased opposing force, and 6 per cent. as loss arising from the increased useless mass of matter put in motion.

Particular instances might be selected which would exhibit a much larger difference than the foregoing between the effects of engines on these two railways; as, for example, Experiment VI. of the *Æolus*, and Experiment II. of the *Apollo*, with loads and at velocities nearly identical, and giving nearly similar results, compared with Experiment II. of *Bury's engine* at the same speed, viz. 29.8 miles per hour, shew an advantage in favour of the latter of 50 per cent. on both the mechanical, and commercial effects. The results of Experiment II. on the *Venus*, compared with Experiment III. on the *Harvey Combe* at velocities somewhat greater than the last cited, are also in favour of the London and Birmingham engine, though in a less ratio, being 10 per cent. on the mechanical, and 30 per cent. on the useful effects. These instances are adduced simply as confirmatory of the mean in Table XV., though single comparisons, unsupported by repeated trials at the same velocity, are not to be regarded, as indicative of the degree of economy, with the same trust as an average from numerous experiments.

The respective economic qualities of engines are clearly shewn by the last column of Table XIII. The *Neptune*, Experiments I. and II., exhibits the acquisition of a higher sum of momentum, with equal power, at 23½ miles per hour, than the *Lion* at 22½. The *Neptune*, also, exceeds the *Premier* on the mean of experiments at similar velocities. The *Vulcan* performed considerably better work at 26.90, than the *Leeds* at 26.70 miles per hour; the *Æolus* and *Apollo* gave equal results at 29.8 miles per hour; the *Venus* exceeded the *Æolus* at 22.50 miles per hour, &c.

It will be apparent from Table XIV. that general conclusions cannot be safely drawn from the effects of one or two particular engines. It might be deduced from comparisons on the *North Star* alone, that the sum of resistance increases in a much higher ratio than that of the squares of the velocities; whilst the *Æolus*, on the same railway, exhibits the resistance—as do most of the other comparisons—as augmenting in a ratio somewhat less than that of

the direct velocities. These indications should point attention to the mechanical structure of the engines, provided the enquirer be confident of the accuracy of his data, and that his experiments were satisfactory in other respects—such as their being made over precisely the same ground, on the same day, and as nearly as possible at the same time—experimental conditions absolutely essential for determining the relative excellence of different locomotive engines. Supposing these conditions to have been observed, and the recorded results to have been elicited, it might be inferred that the action of the *North Star* was somewhat imperfect at the time of trial—that its blast-pipe was too contracted —or that some other imperfection caused the consumption of power to be unduly increased at the higher velocities. Comparison 6 and 1 on the *Æolus*, at velocities very little varying from those of 4 and 1 on the *North Star*, shew the former engine to have greatly exceeded the latter in economy of power, and in the production of useful effect with equal power.

Other comparisons shew the *North Star* to have been by far the most powerful engine on the list—but, in most cases, prodigal of power. Experiment I., column 11, Table XIII., exhibits this engine to have realized an absolute mechanical effect of more than double that by *Bury's Engine*, Experiment I., at nearly similar speeds; but column 12 shews that, for equal effect, the former consumed the most power, though at the rather lower speed of the two. Again, the *North Star*, (same experiment,) performed nearly four times the work of the *Atlas*, Experiment III., at equal velocities, and in this instance, with less power for equal mechanical effect *.

Table XV. shews the mean velocity of the three experiments on the *Harvey Combe*, and the three experiments on *Bury's Engine* to be nearly identical; the particular speeds of each trial were also nearly similar. The mean of the three results of each, (column 12, Table XIII.,) shews *Bury's Engine* to have produced 10 per cent. greater mechanical effect than the *Harvey Combe*. This may be called a small difference, but, if the experiments are trustworthy, that small differential effect must have had a cause, and that cause is traceable, perhaps, to the circumstance of the *Harvey Combe* being a six-wheeled coupled engine, whilst the other had four wheels uncoupled. Apart from all consideration

* Mr. Robert Stephenson's engine may have been equally powerful with the North Star, as, at 14 miles per hour, it dragged so heavy a load as to justify an opinion that its tractive force would equal the latter at 18½ miles, but, for reasons given before, the evaporative data are not sufficiently correct to permit a safe deduction for consumption of power.

of the respective advantage, or disadvantage of these two methods of construction for tractive purposes, it is important, and not difficult to determine the respective consumption of power consequential to them.

I adduce these few comparisons to exhibit the facility and certainty with which they are developed by this method of investigation. Another instance of the delicacy of this test will be useful to shew the necessity of being informed of every circumstance which may influence the results of locomotive experiments. By referring to the two on the *Soho*, and comparing the momentum generated by 1 lb. of steam at the higher velocity, it appears that this engine realized a much less performance than any other on the list at the same speed; and, on the contrary, that its performance at the lower velocity greatly exceeded every other at that speed. Seeking for an explanation in Mr. Edward Woods's detailed report of those trials, the cause is distinctly made known. He notes, at the higher velocity, the engine then travelling from Manchester to Liverpool, "weather very wet, a strong head wind from the westward;" and on its trip from Liverpool to Manchester, same day, with a luggage train, "weather wet, wind westerly, but not so strong as in the previous experiment." In the one case, therefore, the engine was materially retarded, and in the other assisted by the wind. The augmented consumption of power at the highest velocity to produce equal effects, compared with other experiments, is shewn in Table XIV., for the velocities being as 1.40 to 1, and their squares as 1.97 to 1, the ratio of the momenta, or of the mechanical performance realized, comes out as 2.72 to 1, being an increase in the expenditure of steam at the highest velocity very much greater than that exhibited by any of the other comparisons at speeds bearing a similar ratio to each other. The fact, also, is evidenced by the result, that the engine was proportionally more retarded at the highest, than assisted by the wind, at the lowest velocity *.

The disturbance caused by the wind in these two experiments necessarily renders them unfit to be used for the general deductions exemplified in the four Means; and for instituting particular comparisons on the effects of engines, we are, hereby, forcibly warned of the necessity of using those results only which have been obtained under similar circumstances. It is possible that the experiments marked *erroneous* may have been affected by weather, and that the

* Part of the load was taken up the Whiston Incline by a bank engine, for which a deduction is made in computing the mean momentum at the lower velocity.

reported data were correct; but, if such were the case, they are equally unfit for comparison with engines not influenced by those causes.

This method of investigation discloses experimental defects, as well as errors of fact. Of the latter some instances have been noted, and having already, in explaining the objects of Table XIII., referred to the anomalous results of the *Fury*, we have only to turn to the comparative analysis in Table XIV. to be convinced of their inaccuracy, for we there find a less resistance to have been encountered at the higher of the two velocities, and, consequently, that a gain instead of loss of effect resulted. Similar anomalies would be manifested by reducing the other experiments marked *erroneous* to the terms of Table XIV., and by comparing No. II., on the *North Star*, with No. III.; also, No. V., with No. IV.; No. II. of the *Æolus* with No. III., &c.; but one example was sufficient for exposing this class of errors.

In conducting railway experiments—properly so called—it is important to trim the same engine and tender to the same weight in each trial, as any change in the amount of such weight, which is useless load, materially alters the product of useful tractive effect. It will be seen by comparing Experiments II. and V. on the *North Star*, that there is a difference in the weights of engine and tender of no less than 3.33 tons; and in the two experiments with the *Lion* of 3.43 tons. There is a notable mistake in the Æolus, Experiment IV., the difference being 18.42 tons between the gross and useful loads, instead of 28.41 tons, the weight of engine and tender, according to Mr. Wood, (p. 17, of his Report). Either the gross or useful load, therefore, is misstated, and that experiment ought to be excluded *.

The facts practically elicited in railway traffic, and generally well appreciated by the managers of railways, are the best possible indices of the economy resulting from so accommodating the load to the intended velocity, that the engine shall exert, at all times, as nearly as may be, its maximum force. The Tables present so few experiments with the same engine at like velocities, with unlike loads, that but few comparative results are afforded by them, but this view is corroborated by the *Neptune*, Experiments I. and II., at 23½ miles per hour, and is visible even with so small a difference between the useful loads as 5.65 tons; and to a proportionally greater degree by the *Lion*, in which two trials, at velocities nearly identical, the loads varied in weight 28.05 tons. It has been

* This is probably a clerical error, but was not discovered till after the Tables were printed.

before observed that these engines, dynamically considered, produced respectively an equal mechanical effect, with equal power, in the two cases; but, the useful effect, (Table XIII., column 8,) was smaller in each case, with the lighter load, the light load being saddled with the same useless weight of engine and tender as the heavier one, and equal momenta having to be generated by a like expenditure of power, to realize very unlike commercial effects. The skilful trimming of loads is, therefore, an object of no small economical consequence, as is apparent on the great scale of practice by comparing the cost of working merchandize and passenger trains; and it is still more evident by contrasting the locomotive expenses on well filled passenger trains on some railways, with the precarious amount of passenger traffic on others.

Under the head of experimental defects, detected by this method of analysis, may be classed those which arise out of referring positive facts and data to imaginary planes. In the *Leeds*, Experiment I., the engine actually travelled over 29½ miles, at a mean velocity of 18.63 miles per hour, which multiplied into the mass moved, reduced to the terms of column 11, Table XIII. shews an absolute momentum acquired of 2606.60. M. de Pambour in his *new data* for the same engine, using the same experiment, assumes and applies for his ideal plane a velocity of 20.34 miles per hour, which, with the same mass, gives a momentum of 2846.27. He corrects the consumption of water by ⅕th for waste, making its consumption 0.840 lb. per second; which being applied to both cases, the momentum generated by 1 lb. of steam per second is for the 1st, or real case, 3103.09, and for the 2d, or imaginary one, 3388.41. These sums prove the insufficiency of the method of reducing the real facts to the case of an ideal dead level; for, it is impossible that the same load should be moved at a greater velocity and realize a greater mechanical effect, than at a lower speed with equal power. It is to be observed too, that M. de Pambour originally considered the 29½ miles as a level, in consequence of the help given up the incline by the bank engine; so that two different levels, giving necessarily different velocities, have been used by him for the same case; nor does the additional correction for water referred to before, (p. 101 of this paper,) set his *new data* to rights for the newly assumed level.

The same observations apply to the two cases of the *Hecla*, but from the care taken to ascertain, and apportion the consumption of water as steam in those cases, the error is not so great as in M. de Pambour's; still it is considerable, for the mechanical effects produced with equal power at the two velocities

are equal: but, we know that such a result must be inaccurate, and it arises from not applying a correction for the increased resistance encountered at a higher velocity, in the reduction of the acclivities and declivities to an imaginary level. The assumed dead level, as referred to time, may be theoretically correct, but it is clearly erroneous to superinduce that the same load could be moved over the same space in the shorter time, by the same expenditure of power actually required in the longer time; for, it is tantamount to saying that no additional resistance is opposed to an engine at high than at low velocities. The value of a theoretical rule is its sure, useful application to, and agreement with practical facts, an accordance which cannot obtain between the theoretical and practical results of the working of an ideal and actual railway, unless all the circumstances which affect the latter be taken into account. It appears from the experiments digested in these Tables that the sum total of resistance opposed to the steam, increases in about the ratio of the velocity of the engine. Should that fact be substantiated by subsequent, and more careful experiments—or whatever its amount may prove to be—it must be applied to the reduction of levels, or theory will not accord with practice.

The ascertainment of the ratio which the sum total of resistance opposed to the steam, at different velocities of an engine and train, bears to the ratio of those velocities—and whether such ratio be uniform, or variable, for similar differences at all comparative velocities—are problems which can only be solved by practical experiments on the power expended in the production of given effects. Hitherto, no measure of dynamic effect, appropriate to the characteristics of locomotive performance has been discovered, or proposed, to which the expenditure of power, or efficiency of the steam, could be referred, as decisive of the relative perfection of engines, or of the relatively constructive perfection of railways. It has escaped notice that the momentum generated by the power of a locomotive is the characteristic quality of its functions; and that it comprises all the mechanical effect of which any account can be taken, or from which any practical useful result can be drawn.

The economic properties of one engine are strictly comparable with those of another, by ascertaining the momentum which each can generate with equal power, at equal velocities. Upon this faculty of the engine depends the amount of useful tractive effect which can finally be realized.

That engine which, with the least power, generates the greatest momentum at a given speed, is evidently more economical of steam than another engine

producing a less momentum. It is the better engine, and all things being the same, it would drag a greater useful load with the same power, or attain a higher velocity with the same load.

The determination of the performance of locomotive engines by this method, is rendered as practicable, as exact, and as demonstrative of their relative powers and dynamic excellence, as the determination of the duty done by pumping engines; and the phrase *momentum* possesses the advantage, in common with the term *duty*, of being a true expression of a practical effect. The amount of momentum is derived from the mass, and from the velocity given to it, two facts which are more easily appreciated, and liable to less error than the measuring of pumps, lifts, &c., necessary for ascertaining the duty of pumping engines. The water consumed as steam being contained in vessels whose capacity is readily measurable, no difficulty exists in ascertaining the expenditure of power; and the computations for reducing, and comparing the results are of the simplest kind.

Though the work done by a locomotive may be reduced to the terms and expression of *horses' power*, that expression is, itself, a fallacy; and, until some instrumental means of accurately detecting the pressure *within* the cylinder, and on both sides of the piston, be made applicable to this engine, the determination of horses' power must rest on vague data obtained from *without* the cylinder, and can only be based on estimations of resistance which vary with every load, with every velocity, and which cannot be submitted to any positive test of truth.

Effects of a like nature can be obtained on all railways, and, with proper precautions in the conduct of the experiments, results of indisputable exactitude would be obtained, which could not fail to exhibit the precise, and comparative state of excellence of the engines and vehicles; for, not only the scientific effects, but the commercial results, dependent on the constructive perfection of engines and vehicles used on railways, are reducible to a common standard of comparison by this method.

The resistance, (inclusive of friction,) opposed to the progress of one engine and tender is measurable, in respect of another engine, by the power relatively consumed at equal velocities. In like manner, the special resistance of the load is ascertainable, and as that resistance arises, in great measure, out of the form, dimensions, and weights of the vehicles employed, and, as the final commercial results,—it may be said, even, the commercial prosperity—of railways depend,

in great measure, on the adaptation of the vehicles to the transport of the greatest number of passengers, or of the greatest weight of merchandise, for the least weight and bulk of matter set in motion, the importance of an exact method of comparing these respective qualities will be apparent; for, the forms and other properties of the vehicles on one railway, can be assimilated to those on another which may be proved to offer the least resistance to the motive power.

In the last three Tables referred to I have disregarded any comparisons of effect by the consumption of coke. No measure of power is derivable from the weight of fuel burnt, its effect having reference solely to its calorific strength, and to the qualities of the boiler. It is important to know the efficiency of the combustible, but the efficiency of the steam can alone determine the qualities of an engine. If the coke consumed by the engines of the Great Western, and London and Birmingham railways in Table XV., had been made the measure of effect, the results would have been 8 per cent. less in favour of the latter, as the coke consumed was in the ratio of 1 to 0.72, and the water as steam as 1 to 0.64, the Great Western being unity; but on a comparison between all the engines in the Tables, even 50 per cent. would not cover the difference in their evaporative results.

Table XV.

MEAN PRACTICAL RESULTS OF SIX EXPERIMENTS ON THE GREAT WESTERN, AND SIX EXPERIMENTS ON THE LONDON AND BIRMINGHAM RAILWAYS, AT EQUAL VELOCITIES.

	Velocity per hour.	Gross load.	Gross load moved by equal power, at the velocity.	Useful load.	Useful load moved by equal power, at the velocity.	Momentum generated by equal power, per second.	Weight of engines and tenders.	Width of gauge.
	Miles.	Tons.	Tons by 1 cubic ft. water.	Tons.	Tons by 1 cubic ft. water.	Mass × veloc.	Tons.	Ft. In.
GREAT WESTERN	27.02	89.11	0.677	62.33	0.4735	1545	26.78	7 00
LONDON AND BIRMINGHAM	26.90	67.79	0.801	51.00	0.6028	1821	16.79	4 8
	Ratio.	Ratio.	Ratio.	Ratio.	Ratio.	Ratio.	Ratio.	Ratio.
GREAT WESTERN	1.000	1.00	1.00	1.00	1.00	1.00	1.00	1.00
LONDON AND BIRMINGHAM	0.995	0.76	1.18	0.81	1.27	1.18	0.62	0.66
	1	2	3	4	5	6	7	8

ENGINES AND VELOCITIES COMPARED.

GREAT WESTERN.	Table XIII.	Miles per hour.	LONDON AND BIRMINGHAM.	Table XIII.	Miles per hour.
North Star	Experiment I.	18.63	Bury's Engine	Experiment I.	19.42
Do.	IV.	31.52	Do.	III.	31.29
Neptune	III.	29.77	Do.	II.	29.82
Mean		26.64	Mean		26.84
Lion	I.	22.28	Harvey Combe	I.	21.85
Apollo	II.	29.80	Do.	II.	28.53
Venus	II.	30.16	Do.	III.	30.51
Mean		27.41	Mean		26.96
Do. of the six Experiments		27.02	Do. of the six Experiments		26.90

TO PAGE PAGE 142.

TABLE XI

RAILWAY.	NAME OF ENGINE.	No. of experiments.	Mean velocity of engine per hour.	Water as steam consumed per hour.	Gross load.
	Experiments by M. DE PAMBOUR.		Miles.	Cubic feet.	Tons.
	ATLAS.	I.	9.72	43.56	206.90
		II.	15.00	48.30	139.54
		III.	18.15	46.51	52.05
		IV.	24.07	50.87	42.20

TO FACE PAGE 142.

TABLE XIV

OF THE BLAST, AND THE RESISTANCE OCCASIONED BY IT.

The intensity of the pressure subsisting on the opposite side of the piston from the blast has been but imperfectly illustrated by writers on the locomotive engine. The discharge of the steam has been likened to a jet, and that jet considered as continuous, which involves a contradiction, or, at best, a confusion of terms. By the passages subjoined it is clearly M. de Pambour's opinion that the evacuation of a cylinder full of steam has a duration equal to that of a single stroke of the piston; for, to an engine having a blast-pipe orifice $\frac{1}{38}$th of the area of both cylinders, he assigns the velocity at which the steam escapes to be 38 times greater than that of the pistons. We have here an erroneous observation of the phenomena of the blast, and an erroneous determination of its velocity *.

Every observer can appreciate by his ear that an interval exists between the alternate discharges of steam from the two cylinders. The interval between three discharges marks the time which elapses between two successive discharges from the same cylinder; thus, the period between two discharges, from the same cylinder, comprises the time of one discharge, and of two pauses. The velocity of escape is, therefore, as much swifter than that assigned to it by the author, as the difference between the duration of a single jet, and the period intervening between the cessation of one, and the commencement of another jet from the same cylinder. That these jets are periodic, and not continuous, is distinctly evidenced by the audible pulsations in the chimney, even at the very

* "With an orifice $2\frac{1}{2}$ inches in diameter, or 5 square inches area, and cylinders of 11 inches diameter, or 190 square inches total area; that is to say, with an orifice which is only $\frac{1}{38}$th of the area of the cylinders, we see, that in order that all the steam may get out by that passage, its speed in passing through the orifice must be 38 times as great as it was in the cylinder.

"The velocity of the jet formed in the chimney will then be, for the dimension we consider, equal to 38 times the velocity of the piston, or in other words, equal to $6\frac{1}{2}$ times the speed of the engine, the latter speed being nearly six times as great as that of the piston.

"Thus the power of this additional means will be greater in proportion as the velocity of the engine itself will be more considerable," &c.,—"and as that velocity cannot be produced merely by the tendency of the steam to escape into the atmosphere, a part of the power of the engine itself must necessarily in those great speeds, be spent in expulsing the steam; that is to say, in blowing the fire in the fire place," &c.—(*Treatise on Locomotives*, page 247.)

highest velocities of an engine, and their duration may be measured at the lower speeds. Now, supposing—what is, probably, the fact—that, at some certain velocity of the piston, the duration of one complete evacuation, or cylinder full, corresponds with the period of the interval elapsing between it and the next discharge from the same cylinder, it is evident that the velocity of the jet, in that case, would be exactly twice as great as the velocity assigned to it by M. de Pambour's theory.

It is important to have a clear perception of the intermittent action of the blast; for, upon that action depend, in great measure, the resulting pressure against the piston, and the production of a sufficient current of air through the fire, both which effects would be materially changed in intensity, by the substitution of a continuous for a periodic current.

On opening the eduction valve the compressed steam in the cylinder exerts, for an instant, its maximum pressure against the piston, but, by virtue of its elastic force it expands with immense rapidity, and quits the cylinder at a velocity as much greater than that of the motion of the piston, as would be expressed by multiplying the difference between the areas of the cylinder, and of the blast-pipe orifice, into the difference between the time occupied in the escape of the steam, and the duration of a stroke of the piston. Assuming, for an example, that a single discharge is effected in $\frac{1}{4}$th of the time of a single stroke, the velocity of the jet into the chimney would be 19×4 (for the dimension of cylinder and blast orifice cited by M. de Pambour,) or 76 times as great as the velocity of the piston, but the true ratio between them cannot be known, until the exact duration of a jet be determined for different speeds of the piston, and for different acting pressures of steam. It will be observed that the velocity of the jet is here considered as that from a single cylinder, not from both cylinders as by M. de Pambour. Half the steam ejected from the cylinders on the above supposition of the periods of blast and of pause, gives twice the speed assigned by the author, and as the jets from the two cylinders alternate, the real velocity of the blast must be calculated on that from a single cylinder.

Although experiments are requisite for determining the precise duration of the jet, or the time occupied by the steam in evacuating the cylinder, its period is ascertainable within a definite limit.

The ear and the eye distinguish the lapse of a sensible interval of time between every two discharges, and these discharges proceed from two cylinders. The two engines—for the locomotive carries two—are so combined that a dis-

charge from the one commences at the half stroke of the piston of the other, or at periods definitively marked by spaces of 90 degrees on the revolution of the crank shaft, during which, four complete blasts occur. The consequence is that we know a single discharge to be completed within the time occupied by the piston in accomplishing a half stroke, or before the piston has traversed half the length of the cylinder; yet, the discharges are audibly distinct, and the pause between one discharge and its successor is perceptible. This pause may equal, or it may be longer or shorter than the duration of the jet, but we see that a single blast cannot occupy the fourth part of the time of the revolution of the crank shaft, and that, very probably, it does not exceed the eighth part, or the period of a quarter stroke of the piston. It must be also borne in mind that the same principle holds good at all velocities of the engine.

Under no circumstances, therefore, does the pressure from the blast oppose the piston much longer than during the fourth portion of its course; its intensity is at a maximum at the instant of the commencement of a return stroke, at which time the piston is at its slowest speed; and its minimum occurs at the instant of the completion of the discharge: the mean of these two pressures—which cannot exceed the half of that of the active steam—operates, therefore, as a retarding force during a period of less than that of the half stroke; and, probably, it never exceeds in pressure the fourth of the intensity of the active steam, reckoned upon the whole stroke, with the usual dimensions of the blast-pipe orifice. Upon this supposition, and with an active pressure of 30 lbs. per square inch, the mean resistance from the blast would not be greater than $7\frac{1}{2}$ lbs.; and, with half that active pressure, or 15 lbs., it would not exceed $3\frac{3}{4}$ lbs. per square inch against the pistons.

Before proceeding to cite some observations and experiments which will tend to corroborate this argument, I may conclude the remarks upon the nature of the blast in urging the fire, the process for which alone it is intended. The evacuation of the contents of the chimney, by the periodic blasts of steam through it, effects this object; a continuous discharge of steam into the chimney would defeat it, as the chimney could not be occupied by steam, and by the products of combustion at the same time. A succession of discharges, and a succession of intervals, are, therefore, essential to the success of the process, for, from the expansive and condensible nature of steam, a temporary and partial vacuum is formed in the chimney on the cessation of a discharge, which is almost instantaneously replenished by the pressure of the air into the fire, and

through the boiler; another discharge takes place, the chimney is again evacuated of its contents, and so on. Thus, the jets of air into the fire alternate with the jets of steam into the chimney.

It is practically found that, at high velocities of the engine, evaporation is accelerated; consequently the fire is urged to more intense action, and the combustion of the fuel is more rapid in a given time. This result is the natural effect of velocity in the engine; for, the number of discharges of steam into the chimney is proportional to the speed of the piston; the number, therefore, of jets of air into the fire is augmented in like proportion: perhaps, also, the duration of the jet into the fire is longer, with respect to that of the jet of steam into the chimney, at high than at low velocities; for, though a cylinder full, or an equal measure, composes the volume of the jet of steam at all velocities, its density varies with the speed of the engine, (under the ordinary circumstances of railway traffic,) and the rate of expansion, or relative duration of a discharge may be influenced by it. The less dense steam may, therefore, occupy less time in escaping from the cylinder, than the more dense steam. Supposing such to be the case, the frequency of the discharges would not only be increased in the ratio of the velocity of the piston, but the interval between the discharges, or the time given for air to enter the fire, would be also increased; whence, the increased evaporative power of the boiler.

It is the frequency of the jets into the chimney which enables a locomotive boiler to vaporize as much water in a given time at a high, as at a low velocity; but, in order that it should vaporize more water in equal times, some additional cause seems to be required, for more air must be admitted to the fire, inasmuch as the combustion of the fuel must be swifter. We must, therefore, suppose either the vacuum left in the chimney at the higher velocity to be somewhat greater than at a lower speed, or the discharge to be more swiftly effected, so as to increase the relative length of the intervals between the discharges. The fact that an equal evaporation can be maintained at high, as at low speeds, shews the jet of lower steam to be equally as effective as the jet of higher steam; and this fact is consistent with the office of the jet, which being little more than that of emptying the chimney of its contents, a cylinder full of steam at 15 lbs., or much less pressure, would be as efficient as the same measure at 50 lbs. per square inch.

The engine under my charge (previously referred to p. 78, and upon the boiler of which, described p. 71, the experiments were made on the

constituent heat of steam) furnished a favourable opportunity for investigating the effect of the blast in increasing resistance. The cylinders were 10 inches diameter, stroke 24 inches, velocity of piston 240 feet, or 60 double strokes per minute. The cylinders being fixed horizontally on each side of the fire-box of the boiler, and the blast-pipes joined to their under sides, the latter were not less than 10 feet long from their junction with the cylinders to their termination in the chimney. These pipes united at the smoke box, were 3 inches diameter, and the blast orifice was $2\frac{1}{4}$ inches. A fly wheel 7 feet diameter, weighing 14 cwt., and having 65 revolutions per minute, was driven by the crank shaft, the movement of the machine which carried the engines, and to which they gave locomotion, being too slow (viz., 5 feet per minute) to enable the cranks to pass the centres smoothly without additional momentum.

I was able to assign certain loads to the engines which, at the above velocity, severally required $1\frac{1}{2}$, 4, 8, 15 and 20 lbs. pressure per square inch on the pistons to overcome them. The friction of the engines, unloaded, was balanced by $1\frac{1}{2}$ lb. of steam; they stopped with 1 lb. This, like all the other piston pressures here spoken of, is to be considered as denoted by that in the boiler, maintained, on experimental occasions, at the degree which would just balance the load, at the common velocity of 60 strokes per minute. It is probable, at this moderate speed of the pistons, that the elasticity of the steam in the cylinders coincided very nearly with that in the boiler. It has been before stated that a thermometric steam-gauge was attached to the boiler, which gave sure indications of the pressure within it.

The immediate cause of my entering on these experiments is worth mentioning. I one day observed the mechanic in care of the machine, whilst preparing for work, opening and shutting the grease cocks of a cylinder, and giving oil to the piston. The engines were then working without load, and it was evident that a small vacuum existed, after the blast, or the oil would have been blown back instead of entering the cylinder. This fact, the possibility of which had not before struck me, induced me forthwith to order another gauge from Mr. Adie, which was fixed on one of the blast-pipes, in a convenient place for constant observation, about $2\frac{1}{2}$ feet from its junction with the cylinder, the bulb being exposed to the full current of the escaping steam. This instrument detected the fact of a vacuum by marking, usually, a temperature of from 208° to 210°, or about 1 lb. per square inch below the atmospheric pressure, the active steam on the piston being $1\frac{1}{2}$ lb. above it. When the engine was driven at

double velocity, or at 120 revolutions per minute, at which speed it required about $3\frac{1}{2}$ lbs. of steam, the thermometer rose to 211°, and when locomotion was given to the machine at the usual velocity of 60 revolutions of the crank shaft, and requiring 4 lbs. in the boiler, the blast thermometer stood at 212°, exhibiting a pressure equal to the atmosphere only. At 8 lbs. on the piston, a counter pressure of about 2 lbs. was exhibited, at 15 lbs., about 4 lbs., and at 20 lbs. the blast thermometer indicated 6 lbs., beyond which point I was unable to load the engines.

At all these pressures a phenomenon was visible which was to be expected, viz., small and continual oscillations of the mercury in the thermometer, for it is clear that, at the rapidity with which the discharges succeed each other, sufficient time is not allowed for the mercury to assume either the maximum or minimum degree of temperature of any one blast. Nothing could be gathered from a syphon gauge applied to the pipe at the same point with the thermometer, as the mercury was kept in a state of incessant and violent oscillation. I may also notice another fact well known before, but well shewn by the blast thermometer, viz., that, at whatever pressure the steam existed in the boiler, the blast-thermometer always marked the same degree for the same load and velocity, proving thereby that steam invariably assumes in the cylinder of an engine the pressure due to the load. Under all the loads cited I have worked the engines at their regular velocity, with steam in the boiler from 50 lbs. downwards, and the blast thermometer always gave the same indications as when the steam in the boiler was only of just sufficient force to overcome the load. I have also repeatedly worked the engines alone, and under each of the above loads, with greatly varying velocities, and the blast thermometer instantly rose with an increase of pressure in the cylinders, and as instantly fell by partially closing the regulator, thereby reducing the pressure, and with it the velocity of the pistons. It is, thus, manifest that the counter resistance from the blast augments and diminishes with the pressure on the pistons, and that it is independent of the velocity of the piston; for, in all these observations it only rose with the velocity, when the pressure was also increased.

It has been assumed by M. de Pambour, Mr. Wood, and Mr. Robert Stephenson, it is also, I believe, a generally received doctrine, that the pressure from the blast increases with the velocity of the railway locomotive; the active pressure on the piston being presumed to be less at high, than at low speeds. No experiments are stated by any one of these authors to justify their

hypothesis; and an inference, as to blast pressure, founded on the increased evaporative power of the boiler at the higher velocities of the engine, cannot be admitted as consequent; for, increased evaporation simply denotes an effect arising from more frequent discharges into the chimney, an effect which proves nothing as to pressure against the piston. The fact of a greater resistance accompanying the discharge of low, than of high steam, cannot be admitted without proof obtained from demonstrative experiments; for, it seems to be contrary to the nature of steam, and is unsupported by a single argument, or a single experiment.

The "some hundreds of experiments" referred to by M. de Pambour, (pp. 90 and 91, *New Theory of the Steam Engine,*) and which he says, "will be found related in the second edition of our *Treatise on Locomotives*"—but which second edition has not yet been published—were made "by the application of a thermometer, and of an air-gauge or manometer to the pipe through which the steam, after having terminated its action in the engine escaped into the atmosphere." In this respect they resemble the experiments above related, but in the pages cited which contain this passage, the author only states his conclusions that the temperature of the escaping steam truly indicates its pressure, in which we are agreed. He has left us, for the present, in the dark as to the data, and reasoning on which he grounds his assertion (p. 161, *same Work,*) that the resistance caused by the blast increases in the direct ratio of the velocity of the piston. I may here, also, observe that he has left us equally at fault for the data, and experiments on which he grounds his conclusion that the resistance to a train, from the air, increases as the square of the velocity.

The foregoing experiments on the blast were commenced in the autumn of 1836; they were witnessed by many engineers and mechanics; in fact, the two thermometric gauges formed part of the apparatus of the engines; every time they were worked, observations of the thermometers took place, and an experiment may be said to have been daily made; but I have yet to relate two others, of a still more satisfactory nature, on the same engines.

Reflecting on its indications, it struck me that the blast thermometer could not very accurately determine either the maximum or minimum temperature of the steam in the pipe; and that, with still less certainty, could it indicate the mean pressure of the steam, during an entire stroke in the cylinder; for the pipe, with respect to a cylinder, can only be considered as a cock with respect to a boiler. So great is the well known rapidity of expansion, that steam

of a higher pressure, in passing from a denser to a rarer medium, as from a boiler into the atmosphere, instantly falls considerably below the temperature of the boiling point. This is a condition of the issuing steam, but the elasticity within the steam receptacle, whether it be a boiler or a cylinder, cannot be accurately ascertained by the temperature or pressure of the steam without the receptacle—however large the orifice of exit through a pipe communicating with the atmosphere—for the diminishing pressure within the cylinder must always have a relation to the capacity of the cylinder, and to the rapidity of the evacuation. It must also be observed, that the piston of an engine is in motion during the discharge, and that its effect must be, whilst assisting—for however short a time—in the expulsion of the steam, to compress it in some degree. Neither a thermometer, nor any other instrument placed without the cylinder seemed to me, for these reasons, to possess the faculty of truly indicating the pressure of the steam, at any one instant, within the cylinder. I considered the thermometer to have marked, within certain limits, the pressure in the blast-pipe, but not in the cylinder, and as I could not apply an indicator to the latter, I determined on employing another method of arriving at the exact pressure occasioned by the blast. This method was the synthetical one of working the same loads, at the same velocity, both with and without the blast, which would give certain results, and free the question from all possible error, and all doubt. An opportunity occurred for performing these experiments in November 1837; they were tried on loads requiring, with the blast, 20 and 15 lbs. pressure per square inch on the piston, respectively, in the following manner:—

Preparation was made for removing the blast-pipes at an instant's notice, by loosening the flaunch screws, as I well knew an experiment could not be long continued without blast, and with peat for fuel, which I was using at the time. The pressure in the boiler being brought accurately to 20 lbs., and the engines working at their proper velocity, the pipes were released by attendants; the engines, as might be expected, were for an instant accelerated, but with the regulator in my hand, were immediately restrained; the steam soon began to fall in the boiler, the throttle valve was opened in proportion, and when fully opened, the engines making 60 strokes per minute, the thermometric steam-gauge on the boiler exhibited 16 lbs., instead of 20 lbs. required with the blast: 4 lbs. pressure per square inch was, therefore, the true resistance of the blast, instead of 6 lbs. as previously indicated by the blast-pipe thermo-

meter. The engines were allowed to work till the load pulled them up, when the pressure in the boiler was at 14 lbs.

The steam was again raised, and the experiment repeated with similar results.

The load was then changed for that requiring 15 lbs. with the blast, and I commenced the experiment at that pressure, with which the engines freely started their load. The thermometer marked 12 lbs. as the elasticity of the steam when it had unrestrained admission to the cylinders, and the engines their stated velocity. The mean resistance of the blast at this load was, therefore, 3 lbs., instead of 4 lbs. indicated by the blast-pipe thermometer. The engines were pulled up at 10½ lbs.; a repetition of the experiment produced precisely similar results.

The blast-pipes were then replaced, the above loads severally laid on, and the pressures in the boiler and blast-pipe ascertained to be the same as previously indicated.

I have regretted that I did not seize the same opportunity of trying the effects, without blast, at still lower pressures, as I have not since been able to renew them; but they are comparatively unimportant, particularly as respects their bearing on the railway locomotive; the operation, too, was not a little fatiguing and unpleasant, from my being half smothered by the steam and spray which escaped from the cylinders.

It was thus determined, by conclusive experiments, that ⅕th of the power of the engine was absorbed, at a working pressure of 20 lbs. upon the inch, to blow the fire; and the same at 15 lbs. The length of stroke being greater in my engine than in the ordinary locomotive, would give a result at all pressures, probably somewhat lower for blast resistance than at shorter strokes, but the length of pipe would, perhaps, be against them. These experiments require, however, to be much more varied before it can be safely concluded that the same engine loses power from the blast, in the ratio of the pressure upon the piston, which is the result of the experiments cited. The indications of the blast-pipe thermometer seem to me to have denoted that a less proportional resistance occurs at very low, than at very high pressures.

The ingenious mechanic cannot fail to perceive, from the illustrations here presented of the phenomena of the blast, as they affect both resistance and combustion, that considerable economic improvement may be made in its action;

it is a species of *blower* which has the merit of simplicity, but it is a very costly appendage to a locomotive engine.

Recurring to the velocity of the blast at its issue into the chimney, I am able to assign its speed, very nearly, for the engines on which these experiments were made. It has been already shewn to be a condition, that the duration of a single jet cannot exceed that of a half stroke of the engine. A double stroke was effected each second, consequently a half stroke was completed in the fourth part of a second. A small hole was perforated at the lowest point of the under side of the blast-pipes, to allow the condensed water to escape, which formed in considerable quantities in the cylinders and pipes at first starting the engines, and from the following cause. The blast-pipes, on entering the smoke box, passed through 2 feet of water, below the cylindrical part of the boiler, which was always kept full, to cool by its evaporation the part of the smoke box exposed to the current of heated air from the 5 boiler tubes previously described. These holes were near the cylinders, and could be opened and closed at will; a small jet was given out by them, of equal duration with that into the chimney, which served me as a test of the latter, as follows:—Holding a fine steel marker against the gland of the slide valve, I could observe the commencement and termination of the little jet, and mark their instants on the spindle as it traversed, without removing the eye; and, the motion of the spindle being the counterpart of the piston, the duration of the blast was manifested by the marks, on measuring the portion occupied on the entire stroke of this valve spindle. Repeated observations of this kind demonstrated that the discharge was effected in about $\frac{1}{4}$th of the period of a single stroke, or in about $\frac{1}{8}$th of a second; there was, consequently, an interval of $\frac{1}{8}$th of a second between the termination of a discharge from one cylinder, and the commencement of a discharge from the other cylinder, as the eduction valve of the latter would be opened at the half stroke of the former, which half stroke, at the velocity of 60 revolutions per minute, was performed in $\frac{1}{4}$th of a second.

The escape of the steam from the cylinder was, thus, 4 times swifter than the motion of the piston; the areas of the cylinder and blast orifice were to each other as 19.783 to 1, expressing also the ratio of the relative velocity of the steam out of the cylinder, and into the chimney. The steam, therefore, issued from the blast orifice 79.13 times faster than the piston moved in the same time; and the piston travelled 6 inches in $\frac{1}{8}$th of a second; data which

will be found to give 316½ feet per second, or 215 miles per hour as the rate of velocity of a single jet of steam into the chimney.

This determination of the velocity of the blast jet is a mere statistical fact from particular dimensions of engine, working at a given speed. High numbers always astonish, and they are often cited, whether useful or not; but as incorrect methods had been employed to ascertain similar data, and deductions from such data had been made the basis of fallacious reasoning, both as regards the velocity of the current of heat into the chimney, and blast-pressure, no apology is necessary for my stating the foregoing facts. To render them useful, and connect them with the action of the currents of air into the fire, and into the chimney, another series of experiments, and a different method of investigation would be required.

OF THE EXPENDITURE OF POWER FOR A GIVEN EFFECT BY FIXED AND LOCOMOTIVE NON-CONDENSING ENGINES.

But few direct and conclusive experiments appear to have been made on the expenditure of steam, for a given effect, by non-condensing engines. Of the two in Table VI., Experiment III. is the most satisfactory, as I had opportunities of submitting that engine to very frequent trials; nevertheless, the method employed of ascertaining the pressure was not so perfect as might be wished, as I had no indicator. The absolute pressure on the piston, with the engine loaded, was 14 lbs. per square inch*; its friction, unloaded, was overcome by 4.45 lbs., leaving 9.55 lbs. as the effective pressure.

By comparing the volumes of steam and water consumed per minute, by this engine, we have the ratio of 749 to 1, giving the pressure corresponding with it in M. de Pambour's table, as 21.13 lbs. above the atmosphere. This ratio should have been 771 to 1, for perfect agreement between the elasticity deduced from these comparative volumes, and that denoted by the instrument employed; an identity which would be obtained by augmenting the volume of steam by $\frac{1}{40}$th for the ineffective quantity contained in the passages, &c., an amount not considered, as I could not measure it. A nearer approximation to perfect agreement between the indications of the test per volume, and the real consumption of an engine can rarely be made.

* Throughout this paper, the term *absolute pressure* is used in contradistinction to *effective pressure*, and exclusive of the value of the atmosphere, or 14.71 lbs. per square inch, except when specially mentioned.

The absolute power of this engine was 29.32 horses, and its effective force 20 horses, which were respectively accomplished by the consumption of 81.85, and 120 lbs. of water as steam per horse-power per hour.

It is usually found that $\frac{1}{3}$th of the total power is absorbed by the friction of a good condensing engine; a proportion which would shew the Albion Mills engine (Experiment V. Table VI.) to have exerted an absolute force of $62\frac{1}{2}$ horses, obtained by the expenditure of 55.60 lbs. of water as steam per horse per hour; its effective power being 50 horses, and the consumption of water 70 lbs. per horse per hour.

The ratio of expenditure of power, for equal useful effects, between these two engines—so loaded—was, therefore, as 70 to 120, or as 1 to 1.71.

In the Appendix to a highly ingenious and philosophical "Report on Railways" made by my late esteemed friend, Mr. Charles Sylvester, of Derby, and addressed to Charles Lawrence, Esq., of Liverpool, in 1824, will be found a learned disquisition on the relative consumption of power by the above two classes of engine. Mr. Sylvester's knowledge and accurate theoretical analysis of the subject, are shewn by the close accordance of his conclusions with the facts established on these two engines, at their respective working pressures, viz. "that the relative economy of the condensing, and non-condensing engines, will be as the quantities of steam consumed, or as 1 to 1.72." These deductions were drawn from calculations on a condensing engine supposed to be loaded to Watt's standard—similar to the Albion Mills engine—and on a non-condensing engine supposed to be working with an absolute pressure of 15 lbs., or an effective load requiring 10 lbs. per square inch to overcome it, being half a pound more than the one above cited.

Mr. Sylvester then shews that by increasing the pressure upon the same non-condensing, and by enlarging the area of the condensing engine's cylinder and air-pump, so as to maintain the steam in it at an uniform pressure per square inch for all loads, the economy of the former would gradually approach, and finally equal that of the latter; thus shewing the consumption of steam by the non-condensing engine, for equal effects, to diminish with the pressure on its piston. This is, also, an irrefragable argument as will presently be seen, and was employed by the author in advocating the application to the locomotive of a species of atmospheric cooler, or condenser without water, which would, he thought, bring it more quickly on a par with the condensing engine, as respects economy of steam.

Since the period of Mr. Sylvester's investigation, the locomotive engine has assumed definite characteristics; and, to the resistance of the atmosphere, is superadded that arising from the blast-pressure, which has, hitherto, considerably enhanced the difficulty of determining both its net power, and its consumption of steam, for a given effect.

In the numerous comparisons contained in the foregoing pages between the locomotive, and fixed non-condensing engines, the consumption of the latter has been used, together with that of the condensing engine, as the test of the accuracy of the data of resistance assigned to the former by the various analysts; but, accurate determinations of the expenditure of steam by the same locomotive engine on which the values of friction, and blast-pressure were ascertained, as given in the preceding section, enable me to fix its consumption of water as steam for given effects, and thus to narrow the ground of doubt, and to establish still more correct data for ascertaining the real resistance opposed to progressive motion on railways, than such previous comparisons could furnish.

It was demonstrated, in the manner shewn in the last section, that, with the respective absolute pressures of 15 and 20 lbs. per square inch upon the piston, the resistances from the blast were 3 and 4 lbs. per square inch against the piston. I had frequently ascertained the weight of water as steam consumed by the engine when giving locomotion to the mass on which it was planted, with a pressure of 4, and 8 lbs.; and, on completing the experiments with, and without the blast-pipe, I ascertained the expenditure of water as steam, for several hours, with the load requiring 15 lbs. per square inch; this took place steadily at the rate of 1220 lbs. per hour. At that pressure, the absolute power of the engine was 17.13 horses, found as follows:

$$\overset{\text{Area of both pistons. Sq. in.}}{157.08} \times \left(\frac{\overset{\text{Pressure. lbs.}}{15} \times \overset{\text{Motion of piston per min. feet.}}{240}}{33000\ \text{lbs.}}\right) = \overset{\text{Horse-power.}}{17.13}.$$

The water consumed per horse per hour for the absolute power was, consequently, 71.22 lbs.

The effective power balancing the load is thus found:

$$\overset{\text{lbs.}}{15} - (\overset{\text{For friction. lbs.}}{2.50} + \overset{\text{For blast pressure. lbs.}}{3}) = \overset{\text{lbs.}}{9.50} \text{ per square inch; and}$$

$$\overset{\text{Sq. in.}}{157.08} \times \left(\frac{\overset{\text{lbs.}}{9.50} \times \overset{\text{feet.}}{240}}{33000\ \text{lbs.}}\right) = \overset{\text{Horse-power.}}{10.84}.$$

The water consumed per horse per hour for the effective power was, therefore, 112.54 lbs.

It will be observed that I take the friction of the engines at $2\frac{1}{2}$ lbs. per square inch, though they worked, without load, with a pressure of $1\frac{1}{2}$ lb. of steam; but, it must be borne in mind there was a *vacuum* equivalent to 1 lb. on the opposite side of the pistons, which *vacuum* disappeared at 4 lbs. per square inch. The friction, therefore, must be valued as consuming steam of $2\frac{1}{2}$ lbs. per square inch for all pressures upon the piston exceeding 4 lbs.

I will now bring the absolute pressure of 15 lbs., as exhibited by the thermometric steam-gauge on the boiler, to the test of the pressure resulting from the comparative volumes of steam and water expended.

Area of both cylinders $\overset{\text{sq. in.}}{157.08} \times \overset{\text{in.}}{48.25}$ length of double stroke and spaces, $+\overset{\text{cub. in.}}{576}$ contents of passages, $-\overset{\text{cub. in.}}{301.59}$ contents of piston rods per double stroke, $=\overset{\text{cub. in.}}{7892.79}$; which, divided by $\overset{\text{cub. in.}}{1728}$, gives $\overset{\text{cub. ft.}}{4.567}$ as the volume of steam consumed per double stroke; and this sum, multiplied by 60 strokes, gives 274.02 cub. ft. as the volume of steam expended per minute.

The weight of water being, as aforesaid, 1220 lbs. per hour, the volume consumed per minute was 0.325 cub. ft.

The ratio of the two quantities is 843 to 1, equal, by M. de Pambour's Table, to a total pressure of 31.50 lbs.; from which deducting 14.71 lbs. for the atmosphere, (of which no account is taken for the force subsisting on the opposite side of the piston,) we have 16.79 lbs. per square inch, for the absolute pressure on the pistons, instead of 15 lbs. as indicated by the thermometer.

The quantities found for the pressure and water are, however, positive; the volume of steam is, also, positive; but, some of the steam was unavoidably condensed from the exposed state of the cylinders and steam-pipes; and, to bring the pressure deduced from the volumes to perfect identity with that marked by the thermometer, the volume of water requires to be diminished from 0.325, to 0.308 cub. ft. per minute, or by $\frac{1}{19}$th; which difference—on the supposition that the volume test is perfect, at the ratio of 887 to 1, and that the pressure in the cylinder was truly marked by that in the boiler—would be the precise value of the condensation.

We have thus a very near agreement between the pressure on the pistons derived from a test by a thermometer, which accorded with the safety valve, and a test derived from the comparative volumes of steam and water expended.

The consumption of steam per effective horse-power per hour has been

shewn to be 120 lbs. for the fixed non-condensing engine, (Exp. III., Table VI.,) and for the locomotive under review 112.54 lbs., which proves the latter to have been the most economical of the two, at nearly the same absolute pressures; the former being 14, and the latter 15 lbs. per square inch. The friction of the former was 4.45 lbs., and friction and blast resistance together amounted to 5.50 lbs. in the latter. This is a new and, perhaps, an unexpected result; it shews the locomotive from its smaller cylinders, smaller mass in motion, lighter and smaller rubbing surfaces, to be encumbered with little more than half the frictional load arising from the larger cylinder, heavy beam and connecting rod, and more massive apparatus of the fixed engine. It is proper, however, to observe that an economy of the power expended in friction is dearly bought by the locomotive at the expense of durability; for no short-stroke engines will endure like the longer ones, as is strikingly exemplified by the brief existence of the locomotive in a sound and healthy state, compared with the longevity of a house-engine.

Having now found the expenditure of steam, for a given effect, by a locomotive engine working at a pressure of 15 lbs. per square inch, I will apply the data to other pressures, and examine the results; and, first, on the load requiring 20 lbs., at which pressure the resistance for blast was ascertained, on the same engine, to be 4 lbs. on the opposite side of the piston. The nature of this load did not permit a lengthened account of the water to be taken, but there is nothing more certain than that the consumption of steam must be proportional to the resistance overcome; and that if with an absolute pressure of 15 lbs. the engine consumed 1220 lbs. of water per hour, 1425.31 lbs. would be required by the same engine, in the same time, moving at the same velocity, with a pressure of 20 lbs. per square inch. The absolute power of the engine, at this pressure, was 22.84 horses, and the water expended per horse per hour 62.40 lbs.

The effective pressure was $\overset{\text{lbs.}}{20} - (\overset{\text{For friction. lbs.}}{2.50} + \overset{\text{For blast-pressure. lbs.}}{4}) = \overset{\text{lbs.}}{13.50}$; the effective power 15.41 horses; and the consumption of water as steam 92.49 lbs. per horse per hour.

At 30 lbs. pressure per square inch, the absolute power of the engine would be 34.26 horses; the effective pressure $\overset{\text{lbs.}}{30} - (\overset{\text{For friction. lbs.}}{2.50} + \overset{\text{For blast. lbs.}}{6}) = \overset{\text{lbs.}}{21.50} = 24.55$ horses-power. The water expended as steam, at this pressure, would be 1835.95 lbs. per hour, for, if there be one certain property of steam ascertained, it is that its density is proportional to its elasticity; the aqueous constituent of

equal volumes of steam at 30 and 15 lbs. above the atmosphere is, therefore, as 44.71 to 29.71. The absolute horse power would thus be obtained by the hourly expenditure of 53.58 lbs., and the effective power by 74.78 lbs. of water as steam. It will be observed that I have assumed, at this pressure, the same proportional resistance for the blast as was ascertained at 15, and 20 lbs., and, so far, it is a hypothetical case; but, it has been shewn, in the section on the blast, that the principle of that resistance increasing in the same ratio as the absolute pressure on the piston is, within very narrow limits, a necessary condition developed by the phenomena of the blast.

It thus appears that, with increasing absolute pressures, and under increasing loads, an equal effective power is obtained by the locomotive engine with a diminishing expenditure of steam, which proves the truth of Mr. Sylvester's proposition even with the blast; for, at the respective pressures of 8, 15, 20 and 30 lbs., the effective horse-power—in the usual acceptation of the term—is realized with 256.41, 112.54, 92.49, and 74.78 lbs. of water as steam, respectively; at which last pressure it very nearly approaches the economy of the condensing engine.

There is, however, another value necessary for perfect accuracy neglected in the foregoing calculation, viz., the additional friction brought upon the moving parts of the engine by the load, or, more strictly speaking, by the increase of absolute pressure on the piston; for, with the load, the blast-resistance also augments; so that the additional friction is not proportional to the load (as M. de Pambour expresses it), but to the excess of pressure upon the piston over and above that which is necessary to overcome the friction of the engines without load.

Dealing as I am with facts, and ascertained quantities, not with theories, I can assign no value practically found for this additional friction; but, it is nearly certain that it bears a direct ratio to the force which produces friction between those parts of the engine affected by pressure, such as the valves, joints, and slides. For the sake of shewing by how much the neglect of this quantity may influence the results in power, and consumption of water as steam, for a given effect, I will assume $\frac{1}{15}$th—which, from experiments directed to this end, I do not think to be far from the truth—of the absolute pressure, in excess over that which balanced the friction of the engines without load, to be the value of the additional friction, and that it increases in the ratio of this excess. The total friction of 15 lbs. absolute pressure would thus be 3.33 lbs.;

blast resistance, 3 lbs.; effective pressure, 8.67 lbs.; effective power, 9.9 horses; and the water consumed as steam, 123.23 lbs. per horse per hour.

At 20 lbs. absolute pressure, the total friction would be 3.66 lbs.; blast resistance, 4 lbs.; effective pressure, 12.34 lbs.; effective power, 41.08 horses; and the consumption of water per horse per hour, 101.22 lbs.

At 30 lbs. absolute pressure, the total friction would be 4.33 lbs.; blast resistance, 6 lbs.; effective pressure, 18.34 lbs.; the effective power, 20.94 horses; and the expenditure of water as steam per horse per hour, 87.67 lbs.

If this identical engine were put upon a railway, and wheels of 5 ft. diameter applied to its crank shaft, a velocity of 10.71 miles per hour would be attained by 60 revolutions per minute, at which speed the foregoing computations give its power, and effect, for the different pressures of steam; and from the effective—or what would be better termed the disposable—power of the engine, after overcoming all the friction proper to itself, and blast-pressure, the resistance to the progressive motion of a given load could be accurately ascertained, the disposable force being known in horses-power; but, were I to assign 200 tons, or any other load, as the measure of the power of the engine at that velocity with 30 lbs. pressure—for example—a doubt might be raised as to the correct adaptation of such load to the power, and the results might be considered as hypothetical. I prefer, therefore, to apply the same data for the values of friction without load—additional friction—and blast-pressure—to the locomotive engines in Tables VIII., IX., X., in order to exhibit the facility and accuracy with which the sum of resistance to progressive motion can be found when these data are known; and to shew the discordant nature of results afforded by purely theoretical methods of investigation. These results are registered in Table XVI.

The following is an example of the computation; *Hecla*, case 2:—

$$\overset{\text{Absolute pressure. lbs.}}{30.95} - (\overset{\text{Friction. lbs.}}{2.50} + \overset{\text{Additional friction. lbs.}}{1.89} + \overset{\text{Blast-pressure. lbs.}}{6.19}) = \overset{\text{Effective pressure. lbs.}}{20.37} = \overset{\text{Effective H. P.}}{88.31}, \text{ or disposable force; } \therefore \overset{\text{H. P.}}{88.31}$$

$$\times \overset{\text{lbs.}}{33000} = \overset{\text{lbs.}}{2914230} \text{ raised 1 ft. per min.} \div \overset{\text{Veloc. engine per min. feet.}}{2719.43} = \overset{\text{lbs.}}{1071.63} \text{ resistance}$$

overcome, or tractive effort exerted by the engine; and $1071.63 \div 82$, gross load, $= \overset{\text{lbs.}}{13.06}$ per ton, at 30.93 miles per hour, instead of 22.40 lbs. per ton, according to Dr. Lardner's data.

Table XVI.

From Table VIII.	Velocity of engine per hour.	† Absolute pressure on the pistons, per sq. in.	Effective pressure on the pistons, per sq. in.	‡ Absolute power of engine.	Effective power of engine.	Tractive resistance.	Gross load.	Tractive resistance per ton.	Assigned theoretic resistance per ton.	
Exper^{ts}.	Miles.	lbs.	lbs.	Horses.	Horses.	lbs.	Tons.	lbs.	lbs.	
I.	9.72	49.29	33.82	48.93	33.57	1294.44	206.90	6.25	8.	M. de Pambour.
II.	15.00	29.79	19.52	45.62	29.89	747.25	139.54	5.35	8.	"
III.	18.15	20.00	12.34	37.06	22.36	472.13	52.05	9.07	8.	"
IV.	24.07	13.29	7.41	32.54	18.04	281.05	42.20	6.66	8.	"
V.	31.60	15.30	8.89	42.52	24.70	293.08	41.16	7.12	8.	"
VI.	26.90	20.04	12.37	46.18	28.50	397.55	47.41	8.38	8.	"
VII.	18.63	39.19	26.37	62.67	42.16	848.56	95.41	8.89	8.	"
VIII.	26.70	25.62	16.46	58.72	37.72	529.58	44.08	12.01	8.	"
IX.	21.79	26.62	17.20	49.77	32.15	553.30	57.00	9.70	8.	"
X.	23.00	21.95	13.77	43.33	27.18	443.15	64.36	6.88	8.	"
XIII.	20.34	38.50	25.90	67.24	45.23	833.88	95.41	8.74	‡ 9.60	"
XIV.	29.09	21.54	13.47	53.55	33.48	431.59	45.59	9.46	‡ 10.60	"
XV. 1	26.95	31.04	20.43	104.29	68.64	957.35	82.00	11.67	§ 19.64	Dr. Lardner.
XV. 2	30.93	30.95	20.37	119.52	88.31	1071.63	82.00	13.06	§ 22.40	"
* Soho	30.79	33.30	22.10	107.27	71.19	867.05	46.51	18.64	.....	
Ditto.	30.79	51.30	35.29	165.25	113.67	1384.43	46.51	29.98	.....	
1	2	3	4	5	6	7	8	9	10	11

Columns 9 and 10 of this Table bring the resistances opposed to progressive motion, calculated from data of a practical nature, into comparison with those deduced from experiments of a purely theoretic character; but, there are good grounds to believe that the effective, or disposable power of the engines in the Table is yet too highly rated; for, the friction of these engines, without load, would be greater than that assigned, as their pistons are of larger diameter than those upon which the value of friction was ascertained to amount to $2\frac{1}{2}$ lbs. per square inch. It is probable, too, that the additional friction would be

* From Table XIII. † Vide columns 29 and 33, Tables IX. and X. ‡ Vide page 97.
§ Vide pages 112, 113.

greater than $\frac{1}{15}$th in the railway locomotive, from the shocks and strains to which it is exposed. It must, also, carefully be borne in mind that the only datum whence the power can be derived in these experiments, arises from the calculated ratio of the volumes of steam and water consumed; and, that though the former may have been ascertained with near approach to certainty, the second important quantity has been shewn, in many of the reported cases, to exceed possibility; thus, it is probable that in no one of these experiments has sufficient allowance been made for waste, priming, and delays: the absolute pressures so found must, therefore, be regarded as too high, rather than too low, and, consequently, the resistances to motion exaggerated.

Extraordinary discrepances appear by mutually comparing the velocities, and resulting resistances; they forcibly shew the importance of an exact appreciation of the volume of water consumed, when no other test than its ratio to the volume of steam is used to denote the actual pressure in the cylinders. Great as is the difference between the results with the *Hecla*, and Dr. Lardner's estimate of resistance for the velocities, the contrast between Experiment V. (at a still higher speed,) and that engine, exhibits the tractive effort required, as less by nearly 6 lbs. per ton at the higher, than the lower velocity. Reasons have already been given in the analysis of the experiment with the *Hecla*, for considering the effective power of the engine as too highly rated, after the deduction of water for delays, &c., as no estimate could be formed of the waste through the safety valves. The resistances in columns 7 and 9 of this Table are, therefore, still exaggerated.

The power of the *Soho* has been computed at two pressures; 1st, on the presumption that all the water assigned as evaporated, was expended as steam; 2d, on the presumption that $\frac{1}{4}$th was wasted, as by M. de Pambour's approximation; and it will be seen that the deduction of $\frac{1}{4}$th water has the effect of reducing the absolute pressures, and disposable force of an engine in a much greater ratio; an instance adduced to impress upon observers, and experimenters, the all important necessity of an accurate determination of the expenditure of water as steam. With the deduction of $\frac{1}{4}$th, the force of the wind against this train raised the resistance per ton to 18.64 lbs., or $5\frac{1}{2}$ lbs. higher than in the experiment with the *Hecla* at equal velocities. On the presumption that all the water passed as pure steam through the *Soho's* cylinders, the pressure on the pistons must have been 51.30 lbs. per square inch, which I look upon as a very improbable circumstance, and the resistance to progressive mo-

tion 29.98 lbs. per ton—a still more improbable circumstance. The pressure—with $\frac{1}{4}$th water deducted—would be 33.30 lbs. per square inch.

I have reduced several of the experiments on the engines of the Great Western Railway to the terms of this last Table, but the computations are too laborious to tempt a reduction of the series, unless the waste of steam, and pressure in the boilers were denoted; for, as in Experiment XI., Table VIII., the pressures resulting from the ratio of the volumes of water and steam consumed come out, in many cases, much higher than the boiler itself could have sustained, so that, without correction for waste, computations from such data would give no credible result as to power, or resistance.

An accurate determination of resistance to progressive motion can only be made from correct facts establishing, beyond dispute, the disposable force of the particular engine examined; no such facts are yet on record, and until they be ascertained with the same care, and precision, as upon other classes of engine, doubt and confusion must rest upon investigations which, otherwise, could not fail to indicate and illuminate a path to improvement in locomotive science; and it must be almost unnecessary to say that upon the constructive perfection, and economy of the locomotive engine depends the prosperity of railways; for, the locomotive is the continual devourer of costly power, and the continual object of costly repair.

JOSIAH PARKES.

London, 1839.

This paper was partially read on the last evening of the session. It contained the greater portion of the facts and Tables, and all the principles now developed; but as M. de Pambour's last work, the "*New Theory of the Steam Engine*" was not then published, and as the meeting of the British Association for the Advancement of Science might be expected to produce information on the subject, I obtained permission from the Council of the Institution to incorporate in my paper such matter as might seem expedient; for which indulgence I have to express my sincere thanks.—J. P.

III.—*On the Preparation, Properties, and Uses of Turf and Turf Coke.*

By CHARLES WYE WILLIAMS, Assoc.Inst.C.E.

HAVING laid before the Institution specimens of turf in its natural state, as taken from the bog; also specimens exhibiting certain stages of its conversion into a dense coke, or into the form of a solid artificial coal; I now propose to submit some observations on the composition, properties, and respective uses of these different substances.

My connection with the City of Dublin Company rendered me extensively acquainted with steam navigation, and instrumental in introducing it into Ireland, in aid of inland intercourse on the river Shannon; my attention was consequently drawn, several years since, to the substituting turf for coal, as a fuel for steam vessels—principally on the score of economy and convenience—coal being obtained with difficulty, and at a great expense; while turf abounded in numerous districts along the hundred miles of that river over which the steam vessels of the company daily passed. A further inducement was, that its adoption, as a fuel for steamers, would form a valuable and profitable source of employment for the population. The result of the trial has been satisfactory in every point of view.

In the adoption of a turf fuel, no small inconvenience was experienced from its great bulk; and, in wet seasons, from its retaining so much moisture as seriously to detract from its heating powers. My attention was, therefore, first directed to the remedying these two evils, by obtaining a more condensed and a drier fuel.

In pursuing this object I was struck with the meagre accounts which books afforded of this valuable natural product, and the little attention which had been given to it by scientific and practical men in this country. My humble attempt is to remedy this defect.

As to the means of increasing its density, and thus remedying the evil of its excessive bulk, nothing had been effected; neither had any successful effort

been made towards improving the mode of preparing it for the purposes of fuel, notwithstanding the great importance of these objects.

My attention was further drawn to the value of turf (or peat fuel, as it is called in England and Scotland) by the received opinion, that it had not only the power of giving out an intense heat, and with great rapidity; but that it possessed properties which gave it great value when applied to the various processes of metallurgy; and particularly in the working of iron, where the fuel comes in contact with the metal. This led me to pursue the enquiry on another ground; namely, as being likely to supply an improved fuel for the uses of the furnace and the forge.

The well known superiority and high money value of "*Charcoal iron*," (iron manufactured by means of the heat from charcoal, and which is the leading peculiarity in the manufacture of Swedish iron,) gave a further stimulus to the enquiry. Coke prepared from turf, (a pure vegetable charcoal,) ought, it appeared to me, not only to possess heating properties analogous to those from wood charcoal, but to be equally free from those deleterious ingredients which abound in mineral coal.

Such, indeed, is the value and purity of the iron manufactured by the aid of wood charcoal, as compared with that manufactured by coal coke, the process usually adopted in Great Britain, that it may be hoped that by the aid of turf coke the importation of Swedish iron may ere long be rendered unnecessary.

Of the use of turf coke in working iron, many strong testimonials from practical men were given in a tract presented to me by Lord Downshire, whose attention had long been directed to the means of rendering the Irish bogs more available. Its importance for the uses of the forge cannot be overlooked. Much injury is sustained, not more from the use of inferior iron, than from the impurities of the coal and coke with which, in many parts of the country, we are compelled to work it. The extent of this evil and its consequences, cannot, it is true, be stated in figures, but it is not less appreciable on that account. This is well known to all workers in iron and steel; and, when we find an important part of a machine broken, and probably great mischief done, we are apt to censure the workman, when we should rather lay the fault on the iron, or the impure fuel with which the work has been done.

In pursuing the enquiry as to the use of turf and the manufacture of turf coke, I fell naturally into the common error of taking the lower portions of the

bog, in preference to those nearer the surface; and from this circumstance, that the latter, on account of their lightness, appeared unsuited to the purpose; while the former, from their great comparative density, seemed well adapted for producing a coke which could stand the blast. From the lower strata, a coke sufficiently dense could certainly be formed by the aid of suitable coking stoves; but they were found to be too impure, and impregnated with too large a proportion of incombustible and deleterious matter. From the upper strata, on the contrary, and particularly where they were composed of pure bog moss, which had made but little progress towards decomposition and solidification, I obtained an exceedingly pure carbon; giving a very small per centage of useless, and no injurious matter.

This upper portion of the bog, however, was of so light and porous a texture, and so apt to reabsorb moisture, by which its heating properties were much reduced, that it threatened to defeat all my efforts, as it would scarcely repay the labour of cutting and saving, even for domestic fuel; while the lower strata often approached the solidity of coal. The superior density of the latter, it will be observed, had been acquired by the decomposition, and consequent solidification of its vegetable fibre, and still more by the consolidation, through ages, from the mere pressure of the superincumbent mass; often of the depth of twenty or thirty feet. But this great density is obtained at the expense of purity and heating properties, through the addition of many heterogeneous and incombustible substances; which, *pro tanto*, and without reference to their chemical effects, deteriorate the calorific power and usefulness of turf as fuel.

This has been well illustrated by Mr. Griffith, in his analysis of a part of the Bog of Allen, of the depth of thirty-eight feet, as given in the first volume of the "Parliamentary Reports on the Bogs of Ireland." That report states that the upper portions, even to the depth of eight or ten feet, exhibited so "open-grained and fibrous a texture," that the different species of the mosses of which it had been composed were easily discernible: the *Sphagnum palustre* (the lightest of the bog mosses) predominating. This portion of the bog had as low a specific gravity as 356 (water being taken at 1000); and, what is here important, yielding not more than one per cent. of incombustible ash.

As he descended to the lower portions, he found the mass progressively increasing in density, until it shewed a "fracture, conchoidal; lustre, shining;

with a strong resemblance to coal, and susceptible of a high polish," and further, that it was capable of yielding a "very compact charcoal with internal lustre shining." He found its specific gravity increased from 356 to 1236, but accompanied with the drawback, that its incombustible ash had also increased from one to twenty per cent., independently of the injurious tendency of the substances with which it had combined; thus proving that as the bog had gained in density, it had lost in combustible value, weight for weight; and that even as a domestic fuel it was seldom used, "owing to the unpleasant odour it gave when ignited."

Having ascertained satisfactorily, by experiments in the forging of iron, and by analysis, that the upper and lighter portions of the bog possessed the greatest purity and heating power, weight for weight; the difficulty presented itself of combining density with purity, which qualities, in the natural state, are not coexistent. The process I have adopted has enabled me to obtain a coke from the lighter portions of the bog, not only of double the density of wood charcoal, and equal to that of coal coke, but possessing that purity which is so essential in the working of iron.

Analysis of peat and peat coke.

To ascertain more in detail the relative values of the compressed peat, and peat coke, as compared with coal, coal coke, and charcoal, I had a very accurate analysis made by that able experimentalist, Professor Everitt, whose report I here subjoin, as it contains much interesting matter, and many new facts.

"*Report of Experiments on pressed Peat, and on Coke made therefrom, for Charles Wye Williams, Esq.*

Density.—The density or specific gravity of water			1000
Compressed peat, the thinnest and hardest pressed			1160
Ditto, the thicker or less pressed			910
Peat coke, the thinner or hard pressed			1040
Ditto, the thicker or less pressed			913
The resin fuel, (peat, coke, and resin)			1140
Resin alone			1110
The hardest and dry woods, such as oak, ash, or elm, vary from	800	to	585
The lighter woods, as poplar, pine, &c., vary from	383	to	530
Charcoal from hard woods, varies from	400	to	625
Coals vary from	1160	to	1600

"Hence we see, the hardest compressed peat is denser than the hardest woods, in the relation of 1160 to 885; and compared with some of the lighter woods, nearly double. Further, that the coke prepared from the hardest compressed peat is nearly double the density of ordinary charcoal. In common practice, it is reckoned that 100 lbs. of charcoal occupy the same space in measure as 200 lbs. of coke. The peat coke would, therefore, weight for weight, occupy the same space, very nearly, as common coke.

Calorific power.—"The next point of investigation was the calorific power as compared to coal, common coke, and charcoal. The usual method of making assays of this kind, is to burn equal quantities of the respective fuels, and endeavour to ascertain how much water each respectively will raise a given number of degrees, or convert into vapour. But experiments of this sort, unless made on a very large scale, cannot lead to any comparable results.

"It is given in Berthier (*Essais par la Voie Sèche*, vol. i. p. 289) as being the result of accurate experiments, that a given weight of charcoal will raise 78 times its own weight of water from 32° to 212°, or boil off in vapour $11\frac{8}{10}$ths its weight; which data do not differ materially from the results obtained on a large scale by Mr. Parkes. (See his paper in the "Transactions of Civil Engineers," vol. ii. p. 161.) Now we know, from actual trial, that weighed portions of coke, charcoal, &c., used under stills and boilers holding from five to ten gallons of water, will not produce one tenth of this effect. I am convinced of the utter futility of trusting to any such experiments on a small scale, with the view of having anything like an approximation to the true relative values of fuel. I was here induced to adopt the method recommended by Berthier in his work, vol. i. p. 228, in order to obtain the relative values of these fuels.

"It is assumed that the absolute quantity of heat generated during the combustion of any fuel, is in exact relation to the quantity of oxygen consumed, or entering into combination. Hence, in order to ascertain the relative calorific value of fuels, it is only necessary to ascertain the quantity of oxygen each consumes in burning.

"The best mode of doing this, is to mix a weighed quantity of the fuel with a slight excess of litharge, (oxide of lead,) and find what quantity of *metallic* lead is reduced. According to Berthier, (and which also agreed with some trials made by me on the same substances,)

10 parts of pure carbon will give, of lead	. . .	340 grs.
10 parts of good wood charcoal	from . .	300 to 323
10 parts of dry wood . .	from . .	120 to 140
10 parts of good coal coke .	from . .	260 to 285

"It may here be remarked that, assuming the principle to be correct in practice, it is susceptible of great accuracy; for, as every *single grain* of carbon produces thirty-four grains of lead, any error in estimating the lead is reduced to $\frac{1}{34}$th of a grain of pure carbon.

"In subjecting the peat and peat coke to the same process, I found as follows:

10 parts of the peat coke (picked surface peat) gave	. 277 grs.
10 parts of peat coke (lower strata) . . .	. 250
10 parts of pressed peat (uncoked) . . .	. 137

"The above numbers represent the relative quantities of heat which can be produced by the same quantities of each of the fuels; and in cases where quantity of heat alone is the consideration, these numbers will also represent their relative values.

"But *intensity* of heat is often of more consequence than *quantity;* and intensity depends very much on the *density* of the fuel. Thus, charcoal can never produce so high a heat as coke; and, in this respect, the denser peat coke and common coal coke are about equal. These comparisons are quite irrespective of any foreign matter being present which may be injurious to the quality of iron, when the fuel is used for reducing the metal from its ore, or for working iron by fire, or when it is used under iron boilers for generating steam.

"To see how far it was probable or not that the peat coke contained matter likely to act injuriously in this respect, like some coke, I burned portions of it in a variety of ways, but no sulphurous acid smell could, in any case, be perceived: now, sulphur, or the metallic sulphurets, are the usual ingredients in common coke, to which their corrosive effects on iron boilers is to be attributed; and such coke, during burning, always gives perceptible quantities of sulphurous acid gas.

"As the nature and quantity of ash is sometimes of importance, I have also investigated these points with great care. An average of two experiments, where 1000 grains of peat coke were burnt till all carbonaceous matter was consumed, gave $\frac{5}{100}$ for the quantity of ash of a light buff colour.

100 grains of such ash contain—

Common salt	3·5
Silica and sand combined	15·0
Sulphate of lime	22·5
Carbonate of lime	43·25
Magnesia and carbonate of magnesia	15·00
Alumina	0·75
	100·00

"The ash contained no carbonate of potassa, and is remarkable for the large quantity of magnesia present.

"From my trials I am of opinion, 1st. That the peat coke examined by me contains nothing which would, during the burning, be more injurious to iron than wood charcoal, or the best coke—whether it be used to work iron, or under boilers for the generation of steam.

"2d. That it is equal to the best coke, weight for weight. In heating power a little inferior, weight for weight, to wood charcoal where quantity is the only consideration; but where bulk of stowage and high intensity of heat are important considerations, it is superior to wood charcoal.

"THOMAS EVERITT,
"Laboratory, Medical School,
"Middlesex Hospital."

"LONDON, JAN. 18th, 1839."

Observations on report.

The turf which I submitted for the above experiments was from a bog in Lancashire. But, from other experiments, I find the turf from many of the bogs of Ireland exceed it in purity, and contain a much less proportion of incombustible ash.

In considering the foregoing report and analysis, the great density of both the peat and peat coke, though produced from the lighter portion of the turf, from the surface of the bog, is remarkable; the compressed peat being thirty per cent. denser than oak wood, and double that of lighter woods; while the coke is double the density of charcoal, and on a par with coal coke. I may here add, that this density, which is so valuable where intensity of heat is an object, may be still further increased, and with little additional expense, by continuing the operation of pressing.

This being the first time that the results of the *litharge test*, as applied to turf coke, have been communicated in this country, (the value of which, Berthier,

in his elaborate and admirable essay on combustible bodies, has fully established,) I may be permitted to say that its accuracy, and the small amount of practical error to which the process is liable, as shewn by Mr. Everitt, give it a high claim to our attention; although to persons not familiar with the nature of chemical tests, it may not be so self evident. We here see that the extraordinary attraction which carbon has for oxygen, and the power which it thereby exercises of deoxidizing metallic oxides, renders the litharge test the most suitable for determining the absolute purity and calorific powers of the various cokes. The carbon, under a high temperature, uniting with the oxygen in proportion to its calorific powers; while the lead, being thus deprived of that which is essential to its state of oxide, is precipitated in its pure metallic form:—the relative weights, so thrown down, representing the true combustible values of the several cokes.

It will be particularly observed that Mr. Everitt, in stating the quantity and intensity of the heat given out by peat coke, adds, that these are "irrespective of the presence of any foreign matter which may be injurious to the iron." Now we know that many foreign substances do enter into the composition of coal and coke, and do exercise a very injurious influence over iron and steel, in the furnace and forge. In this respect the importance of the peat coke becomes apparent. Iron, when worked by means of this coke, is not only sooner brought to a welding state, but it is found to work softer and with less of that scaling which is so injurious, particularly in the operation of welding. These facts I have proved both in the furnace where large boiler plates are heated, and in the operations of the forge, when even the worst iron was improved in quality.

It is not an unimportant consideration that peat coke may thus be produced from that portion of the bog which has ever been rejected even as a domestic fuel. Again, that it is precisely that description of turf which most abounds in Ireland, and which, in most of the large bog districts, has hitherto been regarded as an absolute incumbrance; alike unfit for fuel, and for conversion to agricultural purposes. This latter arises from its extreme porousness and levity:—its being so far removed from that decomposition which is essential to the vegetative functions of all soils:—and also its susceptibility of the extremes of moisture and drought,—overcharged in wet seasons, and amounting to a mere *caput mortuum* in dry ones.

Tredgold, speaking of turf, says, "its specific gravity varies considerably, and when free from gravel, its quality as a fuel depends much on its *density*.

As a fuel, it may be divided into two kinds; the first is compact and heavy; the second light and spongy"; and of the former, he adds, "this is the best kind." Again, he says, "Peat affords a mild and gentle heat, but it is not a good heat for supplying furnaces for boilers." Now from these observations it is manifest Tredgold experimented on some of the lower strata, which give out an unpleasant odour in proportion to their earthy impurities. It is also evident he had not analysed it with the view of determining the relative purities of the light and heavier kinds, and of its peculiarities in the working of metals, or he would have arrived at directly opposite conclusions.

Tredgold adds, "according to Clement and Desormes, it affords one-fifth the heat that would be given out by an equal weight of charcoal, which nearly coincides with the ratio of Blavier and Miché." This comparison is, however, an incorrect one, as raw turf is here compared with charcoal. I do not, however, hesitate to enter the lists with these celebrated men, and assert that by proper management, turf, even of the lightest description, will be found to give out more effective heat than wood, and turf coke than charcoal. Where, then, the required density can be obtained artificially, we are in a position to avail ourselves of this substance, which has hitherto been so rejected, (merely from its low specific gravity,) and render it available, as a fuel, where high temperatures are required, and particularly in all the operations of metallurgy, where purity of heat is so essential.

Process of manufacture.

The difficulty of the process of converting turf into coke, has hitherto been an obstacle to its application to the uses of the forge. So great is the attraction it has for oxygen that much care is requisite in managing the process: on the one hand, to deprive it of all volatile substances, and convert it into a pure carbon: and on the other, to avoid waste from partial combustion.

The mode by which I have effected these two objects is by the use of large vertical stoves, combining in their action the distillatory with the stifling process. During the expulsion of the volatile substances, a species of distillation is carried on in the ovens; and when charred to the point when combustion would begin, the stifling process is adopted, by which all waste is avoided. The peculiarity of the process effected by these stoves, consists in watching the progress of the coking, which begins at the lower part of the stove, and so regulating the admission of the due quantity of air, as the operation ascends, until the whole is so advanced and the gases so expelled, as to require the

complete exclusion of atmospheric air; the whole is then suffered to cool by mere radiation from the external surface of the stoves.

Each of these stoves is capable of coking a ton weight of turf at a charge, and twenty-four hours are requisite for the operation. Their number may be increased according to the quantity required. In that case, they may be advantageously built in a range, or combined in a hexagon shape, by which economy of space and materials in their construction is obtained. When so placed, the coking and roasting stoves (of which latter I shall speak presently) may be alternated with great advantage in the economy of heat.

In the preparation of turf coke for the purposes of the forge or melting furnace, a further process is necessary to give that density which is so essential. This density is obtained by pulverizing, or bruising, the raw material in a wet state as taken from the bog, and previously to compression. This operation of bruising, may be effected with very little labour, by proper machinery. The effect of this bruising process, is, by the destruction of the fibrous character, to bring the component parts into closer and more intimate contact; the result of which is observable in the high specific gravity thus obtained.

By the union of these two processes, the pulverizing and pressing, almost any desirable density may be obtained; and we have within our reach the power of imparting to this valuable substance, the most essential properties which fuel can possess; namely, the *purity* of vegetable charcoal, with the *density* of mineral coke.

Berthier, in his essay on combustible bodies, and in which he has given elaborate analyses of various kinds of charcoal, coal, and turf, with their respective cokes, observes, Vol. I. p. 347, "With respect to intensity of heat, there is no combustible equal to coke. It produces results in large furnaces which could not be obtained from wood charcoal. In the furnaces for assaying of metals difficult to be melted, it raises the temperature ten pyrometric degrees higher than the latter. This property is owing to its great density."

But Berthier does not appear to have been aware of the possibility of giving to turf coke, a density equal to that of coal coke, which he thus eulogises. I find in the trials made by him on numerous kinds of turf, there were none in any degree approaching to the density of the fuel I have produced.

Believing, then, that the quality of the iron ore, is not a matter of greater importance than the quality of the fuel by which it is reduced to a metallic

state, it becomes a subject of great interest, in many of the arts and manufactures of this country, to enquire into the nature and properties of turf coke.

Nearly thirty years ago, Mr. Griffith, in the Parliamentary Reports on the Bogs of Ireland, characterised those bogs, as "mines above ground." In one of these reports, under date June, 1810, he makes use of the following, I trust prophetic, observation: "It is not at all improbable that the iron founders and other manufacturers in Dublin, who have occasion for a great heat, quickly raised, may, at no distant period, be supplied with turf charcoal (which is superior to any other) for that purpose, from the Bog of Allen. Many interesting experiments have been made in France on this substance, in most of which the result has answered beyond expectation." From that period, however, to the present, nothing has been done towards availing ourselves of those "mines above ground," or turning this valuable substance to any useful purpose beyond that of a domestic fuel.

As the several uses to which peat and its compounds may be used are more numerous than is generally imagined, I will here detail a few of the leading ones, with some observations on their use and application:—

1st. Of turf, as now prepared and sold in Ireland.
2d. Of this description of turf, after dry pressure.
3d. Of the same, after the operation of roasting.
4th. Of the same, after having been converted into coke.
5th. Of the preparation of turf for the manufacture of gunpowder.
6th. Of a denser kind of turf coke, prepared by grinding.
7th. Of turf coke, applied to the purification of coal gas and coal coke.
8th. Of turf coke for locomotive engines.
9th. Of the same, applied to the manufacture of artificial coal, with bitumen, for steam navigation.

1st. Of turf, as cut and sold by the peasantry in brick-shaped pieces.—The first practical impediment to the use of turf in this state, for the generation of steam, is the great quantity of moisture it contains. The second inconvenience is its bulk. As regards the moisture held mechanically in suspension, this is an evil of much greater extent than is generally supposed; and as the loss of effective heating power must be in the proportion of internal moisture which it has to evaporate, the heating properties of turf saved in the ordinary way is not unfrequently deteriorated to the extent of fifty per cent. Indeed, in wet

seasons, particularly when we have to deal with the light spongy descriptions, from their power of reabsorbing moisture, it is often impossible to maintain the due pressure of steam for the proper working of the engines, even by the sacrifice of any quantity of turf.

For this evil the great remedy is, attention during the process of drying and saving; and care in its subsequent preservation for the uses of the ensuing season. Turf should be stacked under cover, a circumstance hitherto wholly neglected by those who lay up annual stocks for the winter months, as in the case of the distillers in Ireland. Any labour or expense, therefore, devoted to this object, will be amply repaid by the diminished quantity consumed, and the increased efficacy of its evaporative powers *.

2d. Of turf, as now prepared in Ireland, after pressure.—With respect to its great bulk and low specific gravity, this may to a certain extent be remedied, and at an expense which will repay the process. When thoroughly dry, turf may be compressed, in large quantities, at a moderate expense, into one-half, or even one-third of its previous bulk: and which compressed state it will retain, provided it be preserved under cover. This was not hitherto supposed to be the case; pressure has not been applied to turf in this previously dry state, as it was supposed that, like a sponge, it would again swell and resume its former bulk. My experiments on this preparation much surprised me, and led to the manufacture of a valuable description of fuel condensed at a cheap rate, and with the aid of dry pressure alone. Where turf is of a light porous character, and where its bulk is necessarily more objectionable, this dry com-

* The following extracts from Berthier's "Traité des essais par la Voie Sèche" will be found much to the point on the subject:—

"Hygrométricité.—Le bois est un corps extrêmement hygrométrique: il absorbe une très grande proportion d'eau dans l'air humide, et lorsque ensuite on le soumet à une chaleur graduée, il abandonne cette eau par portions successives qui dépendent de la température. Selon M. Karsten, des copeaux de chêne parfaitement desséchés à l'air, perdent encore 0,103 de leur poids, à la température de l' ébullition; mais à cette température le bois retient encore une quantité très notable d'eau." Vol. I. p. 234.

"Dessication.—Si la dessication n'a lieu qu'à la chaleur de 100°, la matière retient encore 0,145 d'eau non combinée; mais en portant la chaleur jusqu' à 150 ou 160°, et en la maintenant pendant quelques heures, jusqu' à ce que la poudre commence à brunir, on a le ligneux parfaitement sec; dans cet état il est composé de parties égales de carbone et d'eau, ou plutôt, d' hydrogène et d' oxigène dans le rapport rigoureusement propre à constituer l'eau. Ce résultat est tout-à-fait conforme à celui que MM. Gay-Lussac et Thénard avaient obtenu il y a déja long temps." Vol. I. p. 241.

pression is of great value; as it will be found to have greater durability in the furnace, than would appear due merely to its diminished bulk.

The lower heavy strata, however, cannot advantageously be compressed, as they have a tendency to crumble and lose their consistency; being in fact to a considerable extent already carbonized. The upper and middle strata may, when thoroughly dry, be compressed under an hydraulic press, in which state they are applicable to many purposes. We know that mere density, in wood, has no relation to the quantity of heating power which it is capable of giving out weight for weight; and when equally dry, the lighter kinds of woods give the same heat as the heavier kinds. The same law applies to turf, keeping in view, equal purities,—equal weights,—and equal degrees of dryness. These are important facts when we have to deal with a light, though pure material, such as the upper portions of turf bogs.

3d. Of turf after the operation of roasting.—When it is an essential object to have highly dried turf, this may be obtained by the roasting process, in the vertical stoves, so arranged that a strong current of air is made to pass through the turf, entering at the bottom and passing upwards, escaping at the top and carrying away the moisture. As a high temperature and an adequate current of air are essential to the evaporation of moisture, these drying or roasting stoves may be placed alternately, as already mentioned, with the vertical coking stoves. The heat thus supplied from the latter will be found sufficient to effect the necessary rarefaction of the air, in the roasting stove; whereby a rapidly desiccating heat and current are obtained, without any cost. I have also found this compressed roasted turf to be well adapted for many kinds of furnaces.

The object of this roasting, is not only to expel the moisture which cannot be dislodged by mere atmospheric exposure, however dry the air may be, but also to vaporize and expel the ligneous acid which abounds in the lighter description of turf. This acid being a great enemy to heat, must be thoroughly dissipated, if we would obtain the greatest calorific effect from the turf. This very circumstance having been hitherto disregarded, has in many instances been the direct cause of its heating properties being underrated. Berthier, who has treated of the relation between desiccation and calorific power with great exactness, states, that having dried wood in a stove during many days at a high temperature, and, further, until it had the appearance of roasted coffee, found

that the calorific power increased progressively as this species of distillation proceeded, up to the degree of heat capable of decomposing it.

Rumford, with the view of thoroughly drying it, cut the wood into thin pieces, and exposed it in a stove for twenty-four hours, and until it began to turn brown, at a temperature of 128°. In all these particulars I have ascertained that turf and wood are perfectly analogous.

Dr. Ure, in speaking of the mischievous effect of moisture in wood, explains it in his usual clear, comprehensive, and satisfactory manner: and how forcibly does all this apply to the moist state in which turf is now usually applied as fuel; yet how indifferent are the consumers to this self-evident deterioration of the heating properties of their turf *.

4th. Of turf coke.—This dry-pressed turf, when coked, forms a valuable fuel, particularly for the use of air-furnaces, in which iron plates for boiler-makers are heated. In the manufacture of marine boilers, and particularly for boilers made on the Cornish plan, many of the plates of iron forming the windings of the internal flues, have to be brought to a red heat above 50, or even a 100 times, before they are fashioned and fitted to their proper places. It is needless to mention the injurious effects of bad sulphurous coal or coke during such repeated heatings, or the advantage of a pure carbonaceous coke, like peat coke, by which, from its freedom from impurities, the quality of the iron is positively improved. I can speak from experience on this head, having had abundant proofs of its value, and having found that the iron heated in a furnace by this description of turf coke, has become softer, more pliant, and malleable.

5th. Of turf, for the manufacture of gunpowder.—The lighter portions of

* Dr. Ure, in speaking of moisture in wood, observes, "Moisture diminishes the heating power in three ways:—by diminishing the relative weight of the ligneous matter:—by wasting heat in its evaporation:—and by causing slow and imperfect combustion. If a piece of wood contain, for example, 25 per cent. of water, then it contains only 75 per cent. of fuel; and the evaporation of that water will require $\frac{1}{28}$th part of the weight of that wood. Hence the damp wood is of less value in combustion by $\frac{8}{28}$ths or $\frac{2}{7}$ths, than the dry. Woods which have been felled and cleft for twelve months still contain from 20 to 25 per cent. of water. It may be assumed, as a mean of several experimental results, that one pound of *artificially* dried wood will heat 35 pounds of water, from the freezing to the boiling point: and that a pound of such wood as contains from 20 to 25 per cent. of water will only heat 26 pounds of such water to the same degree. The value of different woods for fuel, is inversely as their moisture; and this may easily be ascertained by taking their shavings, drying them in a heat of 140° Fahrenheit, and seeing how much weight they lose."

the turf, when coked, and ground very fine, and which is effected with great facility, appear peculiarly applicable in the manufacture of gunpowder. This is owing to its purity and freedom from earthy matter, and also from the extreme pulverulency of which it is susceptible.

Having submitted this coke to the examination of M. D'Ernst, the artificer in fireworks for Vauxhall, his report is very satisfactory. He states that it is twenty per cent. more combustible than oak charcoal, and recommends its adoption in the manufacture of gunpowder. The turf coke was put to the severest test, namely, in the production of coloured lights.

6th. Of the dense, wet-pressed turf coke.—Where a very dense coke is required, an additional process becomes necessary in the preparation of the turf. As already noticed, turf for this description of fuel is taken wet, as cut from the bog, and in that state, ground or pulverized, so as completely to destroy its vegetable fibrous texture, and break up the numerous cells or vesicles in which the moisture is contained. This bruising process may be effected in the common horse mortar-mill. When a large quantity is required, the number of bruising rollers may be increased on the horizontal arms. A mill of twelve feet diameter, mounted with four metal or stone rollers, working vertically on their edges, is capable of preparing ten tons daily. The only attention required is that of one man to supply the floor of the mill with the wet turf, from which, after having made a few circuits of the mill, it passes out sufficiently pulverized for the operation of pressing.

On being so bruised, and while in its wet pulpy state, it is subjected to a powerful pressure, in layers of three or four inches thick, with the proper alternate absorbents or recipients; and on the water being pressed out, it is reduced to the state of thin cakes according to the degree of density required. These cakes are then prepared for the coking stoves by a short exposure to the air, under cover, or by means of the roasting stoves already mentioned.

An hydraulic press of 400 tons pressure can effect the proper compression of one ton of peat per hour. The charges of turf are previously prepared apart, on suitable trucks, and rolled under the press successively as the pressure is completed. The object of this arrangement is to save the time of the press, which would otherwise be wasted were it to stand idle while the turf was being placed in its proper layers.

7th. Of purifying pit coal by means of turf.—Another advantage, and one likely to be of great importance in the arts, arises from the purifying effect of

turf on pit coal, in the process of coking the latter, and in the manufacture of gas for illuminating purposes.

By mixing turf with pit-coal in the coking oven, a remarkable effect is produced, and a very superior coke is the result. The sulphur, which abounds in coal, and which vaporizes and passes away at a temperature even below the boiling point of water, is first expelled by the direct radiation from the turf, which begins to carbonize and give out its heat long before the coal.

Another valuable effect is produced by the union of the ligneous acid of the turf with the volatile alkali, (ammonia,) which is generated in the process of coking coal, and which is always found where nitrogen (its main constituent) is disengaged in connexion with a combustible containing hydrogen. In the process of coking coal, ammoniacal gas is evolved in large quantities. When carried on in close retorts, this ammoniacal gas passes away in a liquid state, being combined with aqueous matter, and forming the ammoniacal liquor of the gas works.

Now ammonia is not only a great enemy to flame and heat, but causes a serious injury to iron. The ligneous acid of the turf unites with and neutralizes the ammonia; greater purity, and consequently increased calorific effect, is thus given both to the coke and to the gas.

Dr. Ure states that, "the gas from coal, as it issues from the retort, is not fit for the purposes of illumination, as it contains, with other matter, steam impregnated with the carbonate, sulphate, and hydro-carburet of ammonia." If not suited for the purpose of illumination, neither is it suited to the purposes of heating and evaporation. The ammonia being dissipated in the early process of coking, is an important circumstance, from its being thus prevented entering into those numerous and injurious compounds of which it is susceptible; or of carrying off any sensible portion of carbon or hydrogen, with which it would readily unite.

As ammonia in combining with acids generates cold—frequently intense cold, its abstraction is on this account desirable. Professor Brande informs us, that "ammonia mixes with the gaseous acids in several proportions; combining with half its volume,—or with an equal volume,—or with two volumes:" an ample range is thus obtained for effecting this union of the ligneous acid with the ammonia through all the stages of their generation in the ovens. That this is the fact, is perceptible in the dense white cloud, (the form which it assumes,) and which passes off copiously during the whole of the operation.

Had it been otherwise, it would have remained until a more advanced stage of the process, when both the turf and coal would have been carbonized, and in which case the superior affinity which carbon has for ammonia, in preference to all the other gases, would have resulted in a serious loss—the carbon. Now carbon is the most important element in all fuels, being, as Dr. Ure appropriately terms it, "the main heat-giving constituent." Its absorption and loss are, therefore, above all things to be guarded against. The investigation of these interesting details remains yet to be pursued by chemists.

8th. Of turf as a fuel for locomotive engines.—The turf coke, in both states of compression, is expected to be applicable to the use of railroad engines, particularly where coal coke is either expensive or inferior in quality. Much must, however, depend on the money value at which it can be produced; and this, also, must have reference to the locality where it may be manufactured. The application of turf coke to locomotive engines has not hitherto been practically demonstrated, but doubtless will in the course of the present year. Ordinary coal coke exercises a most injurious influence on the tubes and fire-boxes of locomotive engines.

9th. Of turf coke in the manufacture of artificial coal.—The last application or modification of this material to which I shall allude, is in the formation of an artificial coal by its union with bituminous matter, as a fuel for steam navigation. This fuel is not a compressed turf, as has been erroneously stated. It is, in fact, an artificial coal, composed of the best ingredients of which pit coal is formed by nature, yet free from any admixture with those substances which deteriorate the heating properties of the latter. This artificial coal is composed of turf coke combined with resin or pitch, according to the desired quality; the former, of course, making the strongest and best fuel.

Natural mineral pit coal we find composed of, 1st, bituminous matter;—2dly, carbonaceous matter;—and, 3dly, of foreign matter of various kinds, and in various proportions, among which are sulphur and the metallic sulphurets, and incombustible earthy substances. In the composition of artificial coal, I have taken resin, as the purest available bitumen, and turf coke, as the purest vegetable carbon. The result of their chemical, or, perhaps, only mechanical union, is a coal, possessing the greatest heating powers in the smallest bulk—avoiding, on the one hand, an excess of bitumen and a deficiency of carbon, as is found existing in cannel coal,—or, on the other hand, an excess of carbon and a deficiency of bitumen, as is found to be the case with anthracite coal;

neither of which, for these reasons, is adapted for the furnaces of marine boilers.

Resin, notwithstanding its high price, is now much used in steam navigation, from its containing so much heating power, and giving out so much flame; it is only used, however, by mixing it with fine coal or cinders, as, alone, it is not an available fuel, notwithstanding its high combustible properties. This arises from the circumstance of its melting at so low a temperature, and so large a portion of it, therefore, passing off unconsumed and in a state of vapour, from the difficulty with which it enters into connexion with the atmosphere during the process of combustion. The union of resin, however, with the carbon of the turf has the effect, by virtue of the extraordinary attraction of the latter for oxygen, of furnishing the necessary supply to the resin, and thus giving full combustible and calorific effect to that powerful bitumen. This may be said to take place, more or less, in the combustion of all coal, and forms the main distinction between bitumen and coal.

In the manufacture of the resin fuel, the turf coke, in the state of powder, is added to the resin, or other bitumen, in a melted state, upon which an immediate and intimate union takes place. The proportion in which these ingredients, when pure, should be mixed, is that of equal quantities, by weight.

The price at which this fuel may be manufactured will necessarily vary according to the locality of the manufacture, and the value, quantity, and quality of the ingredients used; keeping these in view, the cost of the manufactured article may average from twenty to forty shillings per ton.

Having tested the value of this fuel in repeated voyages across the Channel, and during five voyages across the Atlantic in the "Royal William" and "Liverpool" steam ships, the result is, that the most economic mode of using it appears to be in the proportion of 2 cwt. to 20 cwt. of coal. If used judiciously and in this proportion, 20 cwt. of coal, and 2 cwt. of the resin fuel, will do the work of 26 cwt. of coal. The proportion of the resin fuel may be increased with good effect, should the pit coal be inferior in quality or of a slow burning nature.

But this saving of dead weight carried, amounting to above 15 per cent. of the entire supply for the voyage to New York, is perhaps the least important of the advantages arising from the use of this fuel. In case of a protracted voyage, some of this fuel, in connexion with cinders and a small portion of coal, will be adequate to keep the engines in operation. Again, by the suddenness of its action and the great volume of flame given out, it enables the

engineer to avoid a deficiency of steam at one period, by which the engines are retarded; and an excess at another, by which a waste of fuel takes place; this irregularity arising from the difficulty of keeping so many furnaces at a uniform intensity.

Again, where it is desirable to suspend the operation of any furnace, or even all the furnaces under any one boiler, to clear out or repair, a small increase of this fuel under the remaining furnaces, has been found adequate to the maintaining a due supply of steam.

In conclusion, I would observe that the result of ten years' experience, during which I have given much attention to the subject, is, that the turf bogs, particularly those of Ireland, which have hitherto been regarded as an incumbrance, possess many valuable properties which qualify them for several important uses in the arts. The application of an improved fuel to the manufactures of this country, and to the purposes of the steam engine, being a subject of the deepest interest, and as the accumulation of facts is the first step towards the acquisition of sound views and improved practice, I have presumed to submit the foregoing to the notice of the Institution.

This subject being far from exhausted, with the permission of the Institution, I propose, on a future occasion, submitting to them some further observations, with the details of such operations as may then have been proceeded with.

CHARLES WYE WILLIAMS.

LIVERPOOL, February 12, 1839.

IV.—*An Investigation into the Power of Locomotive Engines, and the Effect produced by that Power at different Velocities.*

By PETER BARLOW, F.R.S., Cor. Mem. Inst. France, of the Imp. and Roy. Academies of St. Petersburg and Brussels, Hon. M. Inst. C.E., &c. &c.

My object in this paper is to make a comparison between the actual power expended by a locomotive engine, and the effect produced by that power under different circumstances, particularly at different velocities, and with engines varying considerably in weight and in evaporating power.

I am quite aware of the difficulty of computing the amount of *effective* power due to the piston pressure of a locomotive engine, or, which is the same, of correctly estimating the deductions which ought to be made for the several irregularities and impediments to which such engines are exposed; but the same difficulty is not encountered while we are only estimating the power *expended*, and it is therefore to the latter that I propose to direct the reader's attention. The method I propose to pursue in making this estimation is as follows.

Method pursued. If we know experimentally the number of cubic feet of water evaporated in an hour, or in any given time, by an engine, the space passed over in that time, the diameter of the driving wheels, the length of stroke, and the capacity of the cylinders—we hence know the number of cubic feet of steam which have been produced from one cubic foot of water, and hence, again, by the experiments that have been made on the force of steam by different persons, we know the pressure per inch on the piston; and then making a due allowance for the atmospheric pressure on the piston, the friction of the engine gear, &c., we have left the force which ought to be effective on the piston, and this being reduced to the circumference of the wheel should be equal to the resistance opposed by the load, which, on a level plane, consists of axle friction, road resistance, and the resistance of the atmosphere to the engine and carriages. Unfortunately, however, we know that the effective force is much less than would appear from this calculation, because we are thus assuming,—1st, that there is a uniform steam pressure, and that no steam is wasted;—2nd, that there is no skidding, and no priming; in fact, that the whole power expended, after deducting for the pressure of the atmosphere and friction of engine gear, is usefully employed, which is very far from being the case. Still, however, we

shall, at all events, know by such an investigation, what resistance we ought to be able to overcome, and by comparing this with the best estimate we can make of the actual resistance opposed by the trains, we shall learn what amount of power is wastefully expended, and knowing this, it may assist us in tracing the defects, and probably may lead to a means of remedying the evil.

The experiments I propose to avail myself of in this comparison, are selected from those given in Mr. Wood's Report to the Directors of the Great Western Railway (in which all the above-specified data are given), viz., those made on the North Star and Harvey Coombe engines, these being selected as exhibiting the effects on engines of the greatest and least power of evaporation, the ratio of their powers being as 100 to 56·84. To save the trouble of reference, such columns of the original tables as are requisite for our purpose, are given at the end of this paper, together with additional columns resulting from the computation first indicated. (*See* Table A, page 195.)

It will be observed that two different speeds are given, one, denominated "the mean maximum speed," being that which the engines made between the times when they first acquired their full speed, and the shutting off the steam from the pistons; the other, denominated "the mean speed," is that which is reckoned from the first starting of the engine to the final termination of its motion, but it is the last speed only to which the following deductions refer.

Construction of Table. It may be well, however, before we proceed, to give an example by way of illustrating the mode of calculation above referred to.

First, the *North Star* has 7 feet driving wheels, which may be called 22 feet in circumference, the stroke of the piston is 18 inches, and the diameter of the cylinder, 16 inches. From these data we have as follows:—

$$\text{The number of revolutions per mile} = \frac{5280}{22} = 240.$$

$$\text{The contents of each cylinder} = 3{\cdot}141 \times 8^2 \times 18 = 3618{\cdot}432 \text{ cubic inches}$$

$$= \frac{3618{\cdot}432}{1728} = 2{\cdot}094 \text{ cubic feet.}$$

Contents of both cylinders $= 4{\cdot}188$.

And cubic feet of steam per mile $= 4{\cdot}188 \times 240 \times 2 = 2010$.

In Experiment III. the mean speed is 33·9 miles per hour, therefore

Number of cubic feet of steam used per hour $= 33{\cdot}9 \times 2010 = 68139$.

Now the water evaporated in that time being, by the Table, 136 cubic feet, we have $\frac{68139}{136} = 501$ cubic feet of steam to 1 cubic foot of water.

To ascertain the pressure due to steam of this density, I have employed a Table given by Tredgold in his work on the Steam Engine, computed from the data furnished by Messrs. Southern and Creighton in 1802, from experiments made with great care, and with a view to obtain practical results. To save reference, so much of this Table as is necessary for the present purpose, is also given at the end of this paper (Table B, page 196).

Employing this Table, we find by interpolation that steam of this density has a pressure equal to 115 inches of mercury, or to $57\frac{1}{2}$lbs. per square inch, from which deducting 15 lbs. for atmospheric pressure, and 1 lb. for the friction of the engine gear, there is left $41\frac{1}{2}$lbs. per square inch on the piston, which, being the applied force, ought to be effective towards the traction of the gross load.

The area of the pistons being 402 inches, and the ratio of the double stroke to the circumference of the wheel being as 3 to 22, and the gross load in this experiment 70·6 tons, we have as follows:—

$$\text{The power applied} = 41{\cdot}5 \times 402 \times 3 = 5229 \text{ lbs.}$$
$$\text{The work done} = 70{\cdot}6 \times 22 = 1553{\cdot}2 \text{ tons.}$$

Then the ratio of the applied power to the work done, or the applied steam power per ton $= \dfrac{5229}{1553{\cdot}2} = 32$ lbs.

That is, if the whole steam power expended in this experiment could have been employed without waste, it ought, at the above velocity, to have been competent to overcome a constant retarding force of 32 lbs. per ton.

In a similar way the numbers in the last column of the Table have been computed.

Referring in the first instance to the result worked out above, viz., the third tabulated experiment, it will be seen that the steam power expended per ton of the gross load amounts to 32 lbs., whereas it has been hitherto commonly assumed, that on a level—which this line is very nearly,—the retardation does not amount to more than 9 lbs. per ton, so that there appears to have been expended a power more than three times greater than the mechanical resistance to which it was opposed, at least according to the views hitherto taken of the subject. And it is of the first importance to ascertain whether there is really this waste of power, or whether the resistance to the load is not actually much greater than has been commonly assumed; also whether this resistance is not greater with greater velocities, and if so, from what cause it proceeds. I have

in a subsequent page examined the question of resistance, from which I think it will appear that the resistance has been hitherto taken too small with high velocities; but after all, the great discrepance between the power expended and any estimated probable resistance overcome, is, I think, clearly due to a waste of power in the engine; for it will be observed that the experiment referred to here, is one that gives the most favourable result in the first set of experiments, for what may be termed high speed. The first tabulated experiment, which does not give a greatly increased velocity when treated in the same way, shows an expenditure of power amounting to 72 lbs. per ton, being eight or nine times greater than the usual admitted resistance.

In the smaller engine the power expended per ton does not appear so great, but neither is the maximum speed so great; it ought, however, to be observed that the latter experiments were made on a line having steeper gradients than those on the former, and consequently the traction per ton, including gravity on the ascending planes, was considerably more than that merely due to the friction of the load; so that it appears there was upon the whole much less power wasted here than in the former case. Now to what is this to be attributed? Is it due to the different proportions in the parts of the two engines, or to the difference in the velocities; or has such weight and power been given to the *North Star* as surpass what can be advantageously employed? These are questions which appear to me well deserving of serious consideration.

I have before observed, and I again repeat, that I do not offer the above calculations as strictly correct; yet if any thing definite be known of the power of steam, the results ought not to be far from the truth. I am aware also that I have made no deduction for the resistance of the blast pipe, which is perhaps considerable; I have seen it estimated at 3 lbs. per inch on the piston, and it may even be more, it may perhaps be greater than is really necessary. This again I conceive is another important question, particularly as it is obvious from what has been stated, that it is not the want of power in the engines at present employed, but some defect in making use of their power, which at this time limits their effective operations, more especially in the accomplishment of great speed.

ON THE RESISTANCE OF RAILWAY TRAINS AT DIFFERENT SPEEDS.

The resistances to which the engine and carriages of railway trains are exposed may be classed under the following heads:—

1. The resistance of the atmosphere.
2. The friction at the axles.
3. The road resistance.

So much has recently been said on the subject of atmospheric resistance that I am induced to consider this question first, and I shall again avail myself of Mr. Wood's experiments—namely, those made on the Whiston and Madeley planes.

These consist of the means of three sets of experiments, as follows, viz.—

1st. A train of four carriages weighing 15·6 tons, obtained a uniform velocity of 31 miles per hour on a plane descending 1 in 96.

2nd. A train consisting of the same four carriages loaded, weighing 18·05 tons, acquired a uniform velocity on the same plane of $32\frac{1}{4}$ miles per hour.

3rd. A train of four similar carriages weighing 18·05 tons, obtained, on a plane descending 1 in 177, a uniform velocity of 21 miles per hour.

In all these cases, as the motion was uniform, the accelerating force and the retarding force must have been equal, and the former being given by the slope of the plane, the whole retardation is known; and it seems only necessary, by a comparison of these results, two and two together, to separate the different effects from each other.

If the retarding forces above enumerated were all subject to the same law, we should not be able to do this; but, assuming that the resistance of the air varies as the square of the velocity, and that the two latter are constant forces, at all velocities, we can easily separate the effects by a comparison of any two of these results with each other.

First, then, let us compare the second experiment with the third, in which the gross weights are the same, viz., 18·05 tons.

We must here observe, that a part of the descending weight is a rolling mass, we must not, therefore, take the whole weight divided by the inclination of the plane for the accelerating force. To arrive at the actual acceleration we ought to know the weight of the 16 wheels and their axles; this datum, however, is not given, but we shall not be far wrong in taking this weight at about $2\frac{1}{2}$ tons, or as the experiments are only comparative, let us assume the weight at 3 tons in each case, and that the distance of the centre of oscillation, to that of gravity,

from the bearing point, is as 3 to 2, which agrees very nearly with Mr. Wood's experimental determination of these points, (*see* Wood's "Treatise on Railways," page 383). On these assumptions the accelerating force of the wheels being $\frac{2}{3}$rds of their weight, the whole accelerating force in the second and third experiments will be $\frac{17{\cdot}05}{96}$ and $\frac{17{\cdot}05}{177}$. Let $\frac{1}{f}$ denote the ratio of the friction, to the load *producing friction*, which we have assumed to be three tons less than the gross load; therefore, the friction, in both these cases, will be $\frac{15{\cdot}05}{f}$ (classing the resistance Nos. 2 and 3 under the general term friction).

Again, let Φ denote the force due to the atmospheric resistance on a unit of surface, and with the velocity of one mile per hour, then if a be taken to denote the opposing surface, and v', v, the two observed velocities, we shall have the following equations, viz.—

$$\frac{17{\cdot}05}{96} - \frac{15{\cdot}05}{f} = a\,\Phi\,v'^2$$

$$\frac{17{\cdot}05}{177} - \frac{15{\cdot}05}{f} = a\,\Phi\,v^2$$

$$\text{Whence,}\quad a\,\Phi = \frac{17{\cdot}05\,(177 - 96)}{177 \times 96\,(v'^2 - v^2)}$$

Now $v' = 32\frac{1}{4}$ and $v = 21$, and substituting these values we obtain

$$a\,\Phi = {\cdot}304$$

$$\text{Consequently,}\quad {\cdot}304\,v'^2 = 316 \text{ lbs.}$$

$${\cdot}304\,v^2 = 134 \text{ lbs.}$$

These results differ slightly from those of Mr. Wood, which arises probably from our having taken the weight of the wheels and axles too great; but as has been before observed, they are intended only for comparing the results with each other, and the difference is therefore of little consequence. In the article in question these are stated at 329 lbs. and 135 lbs. This is the only comparison made in the Report, and if we stopped here, it would appear to be confirmed by these computations; but let us now proceed exactly in the same way, and compare the first experiment with the second. Here the loads are 15·6 tons, and 18·05 tons; the parts of the loads subject to the friction 12·6 and 15·05 tons, and the accelerating forces $\frac{14{\cdot}6}{96}$ and $\frac{17{\cdot}05}{96}$. Our equations, therefore, now are—

$$\frac{14{\cdot}6}{96} - \frac{12{\cdot}6}{f} = a \Phi v^2$$

$$\frac{17{\cdot}05}{96} - \frac{15{\cdot}05}{f} = a \Phi v'^2$$

Whence we obtain, $f = 96 \times \frac{12{\cdot}6\, v'^2 - 15{\cdot}05\, v^2}{14{\cdot}6\, v'^2 - 17{\cdot}05\, v^2}$.

In this case $v' = 32\frac{1}{4}$, $v = 31$, and consequently, $\frac{1}{f} = \frac{1}{108{\cdot}5}$, which value substituted in the original equations gives

$$a \Phi v'^2 = 87{\cdot}1 \text{ lbs.}$$
$$a \Phi v^2 = 80{\cdot}64 \text{ lbs.}$$

That is, by comparing the second experiment with the third, we find the resistance of the atmosphere at $32\frac{1}{4}$ miles per hour to amount to 316 lbs.; and by comparing the first with the second (proceeding precisely the same way), we find the resistance at the same velocity of $32\frac{1}{4}$ to be only 87·1 lbs. The friction by the former comparison being only 5 lbs. or 6 lbs. per ton, and by the latter nearly 20 lbs. per ton.

Such discrepancies as these, where we ought to have a tolerably close coincidence, can lead us only to one conclusion, namely, that it is impossible with experiments conducted on these principles to draw any conclusions relative to the resistance of the atmosphere to railway carriages at different velocities; and the reason of this impossibility is sufficiently obvious; for, by referring to the above formula, it is plain that every thing depends upon the values of v and v', and these ought to express the relative velocities of the wind and of the carriages, whereas they denote, as above employed, the velocity of the carriages only, and cannot therefore be expected to approximate towards the truth, except in a perfectly calm atmosphere. While, however, we are inevitably led to this conclusion respecting the very unsatisfactory nature of deductions drawn from experiments of this kind, there can be no doubt whatever of the important fact, that the carriages did sustain, from one cause or other, the total resistance stated in the article in question; that is to say, a resistance equal to the gravitating power of the plane, that is, of 345 lbs. with the gross load of 15·6 tons, and of 397 lbs. with a load of 18·05 tons; being a total resistance of about 22 lbs. per ton in each case, at velocities of 31 or 32 miles per hour, and only about 12 lbs. at a velocity of 21 miles per hour. Other experiments lead

us to believe, that at velocities of 15 or 16 miles per hour, the resistance does not amount to more than 8 lbs. or 9 lbs. per ton; others again give the resistance much less than these last numbers indicate, viz., not more than 5 lbs. or 6 lbs.

If this great difference of retardation were chiefly due to atmospheric resistance, we should indeed very soon arrive at the greatest attainable railway speed, because our contention would be with a natural force, which it would be in vain to endeavour to overcome, and difficult even to modify; but, if it only arise out of imperfect mechanical contruction, then we may hope that the same perseverance and energy which have already conquered so many difficulties, may enable engineers to modify and reduce those which at present remain to impede their progress of improvement. Let us, then, examine the question under another point of view.

The resistance to railway carriages consists, as we have seen, of three distinct forces enumerated in a preceding page, of which the first, whatever be its amount, we may consider as varying either exactly or nearly as the square of the velocity. The other two forces are generally assumed to be constant, and consequently, that they are independent of the velocity.

That friction is a constant force, has been proved by numerous experiments, and in none more satisfactorily than in those published by George Rennie, Esq. in the "Philosophical Transactions" for 1829. But, it must be remembered, that in experiments made with this view, the utmost care is always taken that all the machinery employed shall be in the most perfect order, the motions free and undisturbed, and every part of the apparatus immoveably fixed and steady. Now this is very far from being the case with a railway carriage, which is in a continual state of oscillation from side to side, and receives also constant blows and concussions from bad joints or other defects in the road. These cannot fail of bringing straining forces on the axles, and, unquestionably, increasing the amount of friction at those parts; and as these blows and irregularities are greater with greater velocities, we have every reason to conclude, that although in the most accurate experiments, friction is found to be a constant force under a uniform pressure, this law by no means holds good in respect to the axles of railway trains, the pressure on these varying every instant, by the cross strains and percussions to which the carriages are exposed.

With respect to the other retarding force, viz., the road resistance; this, while the motion is smooth and tolerably even, would also be uniform with any given radius of wheel; but this is, as we have seen, very far from being

the case, the effect being here even more obviously dependent on the velocity than in the case of axle friction. Not only is the impediment at each blow at a bad joint, or other irregularity, at least proportional to the velocity, but the number of consequent blows and percussions are also greater, as must be obvious to any one who will notice the difference in the motion at high and at low velocities in a railway carriage. There is also another species of retardation which must be included in the road resistance, namely, the effect of the deflection of the rails. This acts after the manner of an inclined plane, so that even on the most level line, the carriages are constantly, for half the length of the rail at least, ascending a plane; and the quantity of this deflection, and consequently the amount of this retardation, has been shown experimentally to be greater with greater velocities, as will be seen on referring to my Second Report to the Directors of the London and Birmingham Railway.

It may be altogether out of our power to determine the law according to which the friction and road resistance vary with the velocity; but that they are greater with greater velocities is unquestionable, and it is therefore quite erroneous to attribute that increase wholly to atmospheric resistance.

It may be said, that so long as the increased resistance is admitted to the amount stated in the experiments referred to, it is of little consequence from what cause it proceeds, but this I conceive is quite an erroneous view of the question.

If all the observed increased retardation with great velocities proceeded from the atmosphere, we should have an unconquerable impediment opposed to our future progress; but, if the retardation proceeds from the other causes glanced at above, then there is every inducement for engineers to endeavour by improving the construction of their lines, particularly the joints of the rails, in order thereby to overcome, as far as possible, the mere mechanical impediments which at present oppose themselves to the attainment of all the advantages of which the railway system seems to be susceptible. With respect to atmospheric resistance, our knowledge is certainly not so perfect as could be wished, and one reason is, that till the introduction of railways, we had no unobjectionable means of making experiments on a sufficiently large scale; but the velocities now attainable are such as would enable any person to arrive at very exact determinations on the subject, and it is much to be wished that some one would make the necessary experiments.

The best results we at present possess on the subject are those of Dr.

Hutton, as published in his "Tracts;" but the surface of the bodies experimented on were small in comparison with the frontage of a railway carriage or locomotive engine, so that it is very desirable we should obtain new data on the subject.

I may just observe here, that Mr. Smeaton published, in the "Philosophical Transactions" for 1751, a valuable set of experiments on the resistance of the air on windmill sails; and in the same paper he gave a table of direct resistances at different velocities, by a Mr. Rouse, adding his favourable opinion relative to its accuracy. These results, in consequence of the general practical nature of Smeaton's communications, have been, I believe, commonly considered as deductions from experiments and observations; but I find, on submitting them to the test, they are merely theoretical computations from an erroneous theory at that time entertained, that is to say, that the pressure of the air at any given velocity is equal to the weight of a column of air of double the height from which a heavy body must fall to acquire the given velocity.* These numbers are therefore not to be depended on; they are too high, exceeding considerably all those obtained experimentally by Dr. Hutton.

It is to be hoped, therefore, that some person who has the opportunity of doing it, will undertake, with proper apparatus, a set of experiments by which this question (by no means an unimportant one in railway practice) may be satisfactorily settled.

That the aggregate resistance is the main question is very true, but we are not likely to arrive at it in the aggregate, till we can obtain the separate effects; particularly as one part depends upon the surface exposed, and the others upon the weight of the load.

There can be no doubt that the aggregate resistance at velocities of 25 or 30 miles per hour is much greater than has hitherto been supposed, this is proved by the whole of the experiments already referred to; and that this resistance should be better understood is very evident, when we consider that upon it depends the great question of economy with respect to gradients. If, as has been generally assumed, friction was the whole resistance to be overcome,

* The formula from which these numbers appear to have been computed for any velocity, v, is $p = \frac{2a \times \frac{1}{2}}{16}$ lbs. for each square foot; a being the height from which a body must fall to acquire the velocity v.

There is certainly a trifling difference between the numbers thus computed and those in the table, but it is generally very inconsiderable, in most cases not amounting to the tenth of a pound.

and amounted to only 9 lbs. per ton, or $\frac{1}{250}$th of the load, an acclivity of 1 in 250 would double the tractive force required; and according to the probable traffic of the road, a greater or less sum might be advantageously expended in reducing it towards a level.

But if the aggregate resistance on a level be 20 lbs. per ton, or $\frac{1}{112}$th part of the load, then such an acclivity of 1 in 250 would be of much less comparative mechanical disadvantage, and the same sum could not be economically expended in the reduction. When we know the amount of total resistance in parts of the gross load at any given mean velocity, nothing is more easy than to estimate the increased tractive force requisite to ascend a plane of given inclination, and consequently the increased power that must be expended to ascend that plane; but even in this case errors have been made in the estimated effect. Suppose, for instance, a plane had such an ascent that the effect of gravity on it were equal to the friction on a level, and admit for the present that the friction remained the same on the plane as on the level, in this case the force of traction is obviously doubled, and it has in some cases been assumed that therefore the power expended in ascending the plane is doubled also; but this is not the case, the power expended consists of that which is necessary to balance the friction of the engine gear, and the resistance of the air to the piston—*plus* that which is employed in overcoming the friction, and it is only the latter that is double, the former remaining the same in both cases, and this constantly absorbed power, in the general run of engines amounts to full one-third of the whole power consumed; to reckon, therefore, that the expenditure of power is doubled because the tractive force is doubled in ascending such a plane as is here supposed, is estimating the mechanical disadvantage of the plane at much too high a rate.

Again, in descending an inclined plane, it has been sometimes assumed that whereas in ascending we had the whole power of the plane against us, so in descending it is all in our favour; and, excepting the absorbed power which is constant, this is true in theory but not in practice. Regard to the public safety will not allow us to avail ourselves in the descent, of the gravitating power of the steeper planes, and only of a certain portion of it in planes of less slopes. In others, viz., in planes descending 1 in 500 or 700 to 1 in 1000, &c., we may perhaps allow the whole gravitating power of the plane to act effectively, and our velocities, therefore, in the two latter cases to be proportionably increased.

I have had an opportunity of communicating with the engineers of several working lines, and have endeavoured to learn from them what acceleration is allowable in the descent of planes of different inclinations. I have thus learned that the practice is different on different railways; but forming a mean result from the whole, it may be stated that in descending with *heavy loads* planes of $\frac{1}{96}$, $\frac{1}{100}$, $\frac{1}{120}$, it is not safe to descend with a greater mean speed than is attainable with the same load on a level; and that on planes of very considerable lengths, varying from $\frac{1}{400}$ to $\frac{1}{500}$, the speed seldom is allowed to exceed that on a level, by more than one-fifth, but that on planes between $\frac{1}{750}$ and a level, the whole attainable speed is admissible.

It is, therefore, of no avail that we actually possess a power that would give us extraordinary rates of speed, if considerations of safety will not admit of our employing it; nor is it of any advantage that we can descend these planes with the admitted velocities, with less piston's pressure, because the steam which would otherwise escape through the cylinders is now wasted at the safety valves, so that we can claim for the advantage of the gravitating powers of the plane only so much as practical considerations will allow us to employ.

Founded on these facts I have, in the last edition of my "Treatise on the Strength of Materials," given a table showing the mean effect of gradients of different degrees, assuming the friction as 9 lbs. per ton. This is referred to by Mr. Wood in his "Treatise on Railways." It appears, however, that he has quite misunderstood the principles on which it is founded, as he states, that I have "assumed, in all cases, the descending effect to be the same as on a level." I am quite sure this mis-statement is wholly unintentional on his part, and that he will have much pleasure in correcting it in any future edition of his work.*

* I regret the more that this mistake has occurred in Mr. Wood's "Treatise," because a writer in the "Dublin Review," taking advantage of it, has copied a page or two from Mr. Wood's work, in order to dispute the principles on which the proposed gradients are formed in the lines laid down by the Irish Railroad Commissioners. I may therefore, perhaps, be allowed to explain, that the Commissioners having reason to believe that the gradients, as laid down by the engineers, incurred an immense expense for earth work, &c., which the expected traffic would by no means justify, they endeavoured, by a circular issued to the engineers of working lines, to ascertain from practical experience the real amount of the impediments and disadvantages of different inclinations; and it was from a mean of these results that the tables and principles adopted in the Report were founded, and not on mere abstract scientific views, as represented by the writer in question. The table alluded to by Mr. Wood was derived from the same practical data, although it is not so stated in the work itself; the Report, at the time, not having been published.

In conclusion, I ought, perhaps, to apologise for laying these remarks, many of which may be said to be rather practical than theoretical, before a Society abounding in so much practical skill and information. Still, however, I am willing to hope that they may not be unacceptable.

If, as I have endeavoured to prove, the power expended is so much greater than the effect produced, particularly at high velocities, it is certainly very desirable, if possible, that this defect should be remedied; and if the resistance at high velocities *is* so much greater than at low velocities, and that this, in part, proceeds from the imperfections in laying the rails, or in other details in the construction, it will follow, that greater attention should be paid to this part of railway formation. And, lastly, if the mechanical impediments presented by inclined planes or gradients be comparatively less than has been hitherto supposed, greater economy than heretofore may perhaps, in future, be practised in the scale of gradients without any serious effect upon the working capabilities of the road.

Table (A)

Showing the Steam Power expended in Locomotive Engines in lbs. per Ton per Mile, with different Gross Loads, and at different Rates of Speed.

Name of Engines.	Dimensions of Engines.	Weight of Engine and Tender.	Weight of Load.	Gross Load.	Mean Maximum Speed per hour.	Mean Speed per hour.	Cubic feet of Water evaporated per hour.	Cubic feet of Steam per hour.	Bulk of Steam Water being 1.	Applied pressure in lbs. per ton of Gross Load.
		Tons.	Tons.	Tons.	Miles.	Miles.	Feet.	Feet.	Ratio.	lbs.
North Star.	Driving-wheel, 7 feet. Length of Stroke, 18 inches. Diameter of Cylinder, 16 inches.	28·89	15·9	44·79	41·15	38·51	197·7	77405	392	72
		28·74	32·92	61·66	36·97	31·52	163·8	63352	388	53
		29·01	41·61	70·62	38·8	33·9	136·5	68139	499	32
		28·68	53·58	79·26	30·63	25·3	153·6	50853	331	51
		28·57	82·03	110·61	32·81	30·66	200·9	61626	306	36
		28·41	166·36	194·7	23·3	18·63	141·4	37446	264	27
Harvey Combe.	Driving wheel, 5 ft. Stroke, 18 inches. Diameter of Cylinder, 12 inches.	17·5	32·65	50·15	32·88	30·51	83·81	48288	576	29
		17·5	53·45	70·95	32·4	28·53	105·9	45103	425	32
		17·25	64·36	81·61	25·53	21·85	70·66	34579	489	22·7

Table (B)

Showing the Pressure per Inch exerted by Steam at different degrees of Density, or according to its Bulk, in comparison with Water at 60°.

Weight of Cubic foot of Steam.	Cubic feet of Steam to 1 of Water.	Pressure per square inch, in inches of Mercury.	Weight of Cubic foot of Steam.	Cubic feet of Steam to 1 of Water.	Pressure per square inch, in inches of Mercury.
Grains.		Inches.	Grains.		Inches.
254·7	1711	30	810	542	105
292	1497	35	910	479	120
363	1178	45	1110	391	150
427	1022	52½	1317	331	180
483	905	60	1520	288	210
593	737	75	1660	255	240
700	623	90	1910	229	270

Woolwich, 1839. PETER BARLOW.

V.—*Description of a Sawing Machine for Cutting Railway Bars.*

By JOSEPH GLYNN, F.R.S., M. Inst. C. E.

THE advantages of railway bars being of equal and uniform length, and having their ends cut as nearly square as possible so that they may abut truly against each other, are so well known and appreciated that it would be needless to enlarge upon them. To ensure these advantages several attempts have been made, but none have been so successful as the mode of cutting off the ends by means of circular saws; this system has been adopted by the Butterley Company at their Iron-works in Derbyshire, and was first used there in the manufacture of the rails for the Midland Counties Railway. These rails are of the H form, as it is generally called, the upper and under surfaces of the rail being the same in shape, so that either may be used as the wheel-track; they nearly resemble the parallel rails used on the London and Birmingham line, but are much heavier, being 78 lbs. in weight to each single yard.

The circular saw has been extensively used for cutting iron, but to whom the merit of its original application is due is uncertain; the ordinary mode of using it was to have the ends of the rails as they came rough from the rolls, separately reheated, and alternately presented to the saw, measuring off by hand the required length. Under this arrangement much depended on the workmen employed; they were not always careful in presenting the bar at right angles to the plane of the saw, nor were they accurate in the adjustment of the length; the ends thus became distorted, and when laid, were out of line with the body of the rail.

To remedy these defects the sawing machine was constructed, and it has completely answered the purposes for which it was designed. The principle of the action of the saws is the well-known one of a disc of soft iron revolving with great rapidity, acquiring the power of cutting any hard substances applied to its periphery without wearing away even the file marks on its edge. The arrangement of the machine may be thus described.

The axis of the saws and the bed of the machine (which is exactly like that of a slide lathe) are placed at right angles with the line of the rolls, through which the rails are passed; the saws are fixed in lathe head-stocks, and slide

upon the bed so as to adjust them for cutting the rail exactly to the required length. The saws are 3 feet in diameter, and $\frac{1}{8}$th of an inch thick; they have teeth of the same size and shape as those of a circular saw for cutting wood, and they make 1000 revolutions per minute, so that the periphery of the saw moves at the rate of 9416 feet per minute, and consequently the teeth are in contact with the hot iron during so short a period that they receive no injury. In order, however, to avoid all risk, and to counteract the effects of the radiating heat of so large a mass of iron, the lower edge of the saw dips into water contained in a cup or recess cast in the head-stocks. The saw is secured between two discs of cast-iron faced with copper rings of such diameter that no more of the saw-plate is exposed than is necessary to cut through the rail.

The rail as it leaves the rolls weighs about 4 cwt., and is about 17 feet long; it is hastily straightened with wooden mallets upon a cast-iron plate, which receives it as it quits the finishing groove, and it lies right for sawing without any change of position, being parallel to the bed of the machine, and sufficiently hot for being cut. Thus the expense of reheating is avoided, and there is a saving of time and labour. The rest into which the rail is received is almost on a level with the ground, and is drawn forward by means of two racks; the pinions that work these racks are turned by a square axle passing through the centres of them, the axle being turned round by a hand-wheel fixed about the middle of it. Thus the rail is brought in contact with the two saws, and the ends are cut off by one operation. If the saws be sharp and the rail hot this is done in 12 seconds, if not, it requires 15 seconds to do it; the saw leaves a slight burr or fin at the section, which is removed by a file when the rails are cold. It should, perhaps, be stated, that when the rails are taken from the saws and are still hot, they are laid to cool in grooves planed in a thick cast-iron plate to fit their shape, so that they cannot warp or twist in cooling, but this is a refinement in rail-making which has no connection with the sawing machine, and may either be used with it or not.

The pivots or ends of the axles of the saws are of hardened steel in the form of a double cone, which prevents the oil from being driven out of the bearings when at a high speed, the centrifugal force keeping the oil at the joined base of the two cones. The bearings are supplied with oil by syphon cups or lubricators, such as are used for locomotive engines, and the machine is driven by belts, except where the motion is taken in the first instance from the line of

rolls, which is done by bevilled wheels; but the mode of communicating the first motion must depend on circumstances.

The Plate shows an elevation, plan, and end view of the machine (Figs. 1, 2, and 3); a section of the saw with the cast-iron discs faced with copper rings, and its axle, as before described, drawn to a larger scale (Fig. 4); a section of the rail and a portion of the saw, full size, showing the angle at which the teeth act upon the rail (Fig. 5); and two views (Figs. 6 and 7) of the sheet-iron hood which collects and confines the incandescent dust from the saw.

JOSEPH GLYNN.

VI.—*On the Expansion of Arches.*

By GEORGE RENNIE, F.R.S., &c.

THE expansion of solids has excited the attention of mathematicians and philosophers ever since the year 1688, when La Hire investigated the effects of heat and cold upon a rod of iron, down to the elaborate experiments of Mr. Daniell on the law of dilatation of metals between high and low temperatures, published in the "Philosophical Transactions" for the years 1830 and 1831. Between these periods we have the experiments of Ellicot, Smeaton, Roy, Ramsden, and Troughton, in our own country, and Muschenbrock, Borda, Lavoisier, Laplace, Dulong, and Petit, on the Continent. In general, however, their experiments have been directed to the law of the expansion of metals and glass, with reference to standards of measurements, but in no instance to their effects on buildings. Some experiments have been made in France, I believe, on the expansion of stones; and a very ingenious application of the expansion and contraction of iron rods in restoring the side walls of the gallery of the Conservatoire des Arts, at Paris, to their perpendicular position, was made by M. Molard.

Rondelet says, "Apprehensions have been entertained by some for the solidity of iron bridges, arising from the effects of dilatation and contraction by changes of temperature." But experience proves that these effects are no more to be feared in iron than in stone bridges, inasmuch as that the abutments being fixed, the arch has no alternative but to rise and fall according to the increase or diminution of temperature. If the arcs of the intrados and extrados of the voussoirs were of equal lengths, the arcs would move parallel with changes of temperature. But as the radii of the two arcs are different, an opening in the joints takes place and becomes an adequate compensation.

Vicat having observed that the arches of a stone bridge built by him over the Dordogne, at Souillac, were subject to periodical motions, concluded that it was owing to changes of temperature occasioned by the bridge having one of its sides exposed to the north and the other to the south, and concluded that all bridges were subject to the same alteration; but he does not seem to have investigated the elongations or contractions of arches independently of exposure.

In the year 1835, a paper by Mr. Adie on the expansion of different kinds of stones by increase of temperature, was published in the 13th vol. of the "Transactions of the Royal Society of Edinburgh." Mr. Adie states that his attention had been accidentally directed to the increase in the openings of a wall during a long-continued and severe frost which occurred in the year 1826; but it was not until the year 1830 that an interdict of the Dean of Guild, Court of Edinburgh, rendered the rate of expansion of stone a matter of more importance than merely a curious philosophical speculation. The reasons given for interdicting the operations were, "That the pillars had not sufficient strength to support the weight of the front, and that the difference of the expansion of cast-iron and stone was so great, that very prejudicial effects might arise from the use of such pillars in this situation." Mr. Jardine, of Edinburgh, was consulted on the part of the Dean of Guild, and Mr. Adie, sen., was requested by the proprietor of the building to calculate the difference of expansion between cast-iron and stone pillars. Sufficient data were easily got for the expansion of cast-iron, but, "the only experiments which could be found," says Mr. Adie, "are contained in a short notice by M. Destigney in the 7th vol. of the 'Quarterly Journal of Science, Literature, and Art.'" The experiment cannot be found in that Journal.

Mr. Adie further states, that since the commencement of his experiments he had "seen a notice in the 'London and Edinburgh Journal of Science' of a letter read on the 12th of March, 1834, to the Geological Society, by Mr. Charles Babbage, in which the author states, from the experiments of Colonel Totten recorded in 'Silliman's Journal,'* he has calculated a Table of the expansion in feet and decimal parts, of granite, marble, and sandstone, from which he finds the alteration in bulk so great, supposing the strata under the Temple of Jupiter Serapis to expand at the same rate as sandstone, and an increase of temperature equal to 100° to act on them to the depth of 5 miles, the temple would be raised 25 feet."

Mr. Adie then describes a steam pyrometer, and an ingenious mode of making his experiments on the expansion of various kinds of stones and prisms of cast-iron at 180° Fahrenheit, the results of which are given in a tabular form, and from which he concludes, "it is evident that no danger is to be apprehended from a change of temperature affecting cast-iron and sandstone in any great degree, as their expansion, so far as regards buildings, may be considered

* Colonel Totten's experiments on stone cannot be found.

as the same." Mr. Adie found the rates of expansion to be considerably influenced by the humid or dry states of the specimens, so that white marble particularly received a permanent increase of length with increased degrees of temperature.

There appeared to be no connection between the density of stones and their expansibility. In conclusion, Mr. Adie is of opinion that not the slightest degree of danger can arise from the use of cast-iron in buildings on account of the difference of their expansion for all ordinary degrees of temperature.

The introduction of iron in bridges, and the subsequent failure of several of these structures, over the Tame in Herefordshire, over the Tees at Yarm, and the iron bridge at Staines, for example, led to the conclusion that these failures were owing to the expansion and consequent pushing asunder of the abutments; and one of the arguments adduced against the iron arch proposed by Messrs. Telford and Douglas in place of the Old London Bridge, was the utter impracticability of constructing abutments capable of withstanding the expansion of so large an arch.

The same arguments were employed against the arches of the Southwark Bridge. In order, therefore, to satisfy public opinion, as well as his own mind, upon this subject, the late Mr. Rennie requested his son, the present Sir John Rennie, to undertake a series of experiments, upon the effect of temperature, on the arches of the Southwark Bridge. Accordingly preparations were commenced in the month of January, 1818, at the time when the main ribs and diagonal braces rested upon the centres, previous to the driving the iron wedges between the abutting plates and the stone-work, and before any of the spandrils and road-plates had been put upon them. The thermometer then ranged from 32° to 45° Fahrenheit. The first experiment was tried with iron and wooden wedges, both of which gave uniformly the same results.

First Experiments. The measurements were taken to $\frac{1}{40}$ of an inch, the results of which gave, for the first arch $\frac{7}{40}$, centre arch $\frac{10}{40}$, third arch $\frac{7}{40}$, for an increase of 13 degrees of temperature; or $\frac{1}{74}$ of an inch in first and third arch, and $\frac{1}{52}$ of an inch in centre arch, for 1° F. of increase of temperature.

Second Set. Experiments were also tried in the months of August and September of the same year, at a time when the arches were completely clear of the centres. Several others had previously been tried (by means of rods applied to the bearings of the centres, as well as by a spirit-level), though not sufficient to enable us to form any tolerable conclusion.

The total range of the thermometer, during the whole time, out of the sun, was from 42° to 75° = 33° F. variation.

The result of 10 experiments tried upon the 3 arches, with a range of the thermometer, from 54° to 75° = 21° F. variation.

For every 10 degrees of Fahrenheit—for the first arch $\frac{11}{40}$, for the centre $\frac{16}{40}$, for the third arch $\frac{11}{40}$ of an inch; or $\frac{1}{36}$ of an inch in the two exterior arches, and $\frac{1}{33}$ of an inch in the centre arch, for 1° F. in temperature.

The result of 23 experiments, on the third arch, gave $\frac{5}{20}$, or $\frac{10}{40}$ for 10° F. in temperature, or $\frac{1}{40}$ of an inch for each degree.

Mode of Observing. The mode of performing the experiments, consisted in having a stout piece of wood, 3 inches thick, properly planed and levelled, fixed to the top of the ribs of the centre, in three places: one on the east, middle, and west ribs, exactly in the centre of the arch, and about $\frac{3}{4}$ of an inch clear of the iron; fine planed wedges 3 inches wide, and adapted to the above spaces, were applied at different hours in the day, using two thermometers at the same time: one in the sun, and the other in the shade; and the rise and fall by these means were taken to about $\frac{1}{40}$ of an inch.

The eastern ribs seem to be more affected than the western; this of course is to be attributed to the longer continuance of the sun's rays upon the former than the latter; it is, however, upon the whole hardly worth while taking into the calculation.

Third Set. The following experiments, upon the Southwark arch, were tried by three accurate thermometers, by Dollond, graduated to half a degree: one hanging in the open air, another having the bulb immersed $1\frac{1}{2}$ inch in the iron itself, and the third hanging amongst the iron ribs, where the temperature upon the whole was much less liable to the sudden variations of the external atmosphere. To the former wooden block was attached an accurate brass scale, by Dollond, divided into $\frac{1}{80}$ of an inch, and a fine piece of feathered-edged brass nicely fixed to the rib, which, by the rise and fall of the arch, traversed upon the scale, and thus indicated the expansion and contraction with tolerable certainty.

I.

THURSDAY, SEPTEMBER 17th. Commenced in the Morning.

TIME.	Thermometer in Air.	Thermometer in Iron.	Thermometer among the Ribs.	STATE OF ARCH.
At 5 A.M. .	42°	Could not be seen.	42°	
6 .	,,	46°	45	Stationary.
7 .	,,	45	45	Rise just discernible.
8 .	,,	47	50	Risen $\frac{5}{80}$.
9 .	,,	48	51	Do. $\frac{1}{40}$.
10 .	,,	50	53	Do. $\frac{3}{40}$.
11 .	,,	52	55	Do. $\frac{3}{40}$.
12 .	,,	53	55	Do. $\frac{1}{40}$.
1 P.M. .	,,	54	56	Do. $\frac{1}{40}$.
2 .	,,	55	57	Do. $\frac{1}{40}$.
3 .	,,	55	57	Do. $\frac{1}{80}$.
4 .	,,	56	57	Stationary.
5 .	,,	56	57	Risen $\frac{1}{40}$.
6 .	,,	56	57	Do. $\frac{1}{80}$.
				Total rise for 15° F. $\frac{26}{80}$, or $\frac{1}{16}$ of an inch; that is, $\frac{1}{48}$th of an inch for 1° F.

II.

FRIDAY, SEPTEMBER 18th. Still, Foggy Morning—No Wind.

TIME.	Thermometer in Air.	Thermometer in Iron.	Thermometer among the Ribs.	STATE OF ARCH.
At 5 A.M. .	48°	50°	49°	
6 .	50	50·5	50	Stationary.
7 .	50·5	50	50	Do.
8 .	51	51	51	$\frac{1}{80}$ rise.
9 .	52	51	52	$\frac{1}{80}$ do.
10 .	53	52	53	$\frac{1}{40}$ do.
11 .	56·5	53	56	$\frac{3}{40}$ do.
12 .	58	54	57	$\frac{1}{40}$ do.
1 P.M. .	58·5	55	57	0 do.
2 .	59	56	58	$\frac{1}{40}$ do.
3 .	60	56	58·5	$\frac{1}{40}$ do.
4 .	58	56	58·0	$\frac{1}{40}$ do.
5 .	57	56	58·0	Stationary.
6 .	57	55·5	57·5	Do.
½ before 7 .	59	0	59	Falling just discernible.
				Total rise for 10° F. $\frac{18}{80}$, or $\frac{1}{5}$ of an inch; that is, $\frac{1}{50}$th of an inch for 1° F.

III.

SATURDAY, SEPTEMBER 19th. Very quiet, still Morning—No Wind.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 5 A.M. .	57°	58°	56°	Stationary.
6 .	56	58	56·5	Do.
7 .	56	58	57·5	Do.
8 .	59	59	58	$\frac{1}{80}$ rise.
9 .	62·5	61	59	$\frac{1}{40}$ do.
10 .	64	63	60	$\frac{3}{40}$ do.
11 .	64·5	63	61	$\frac{1}{40}$ do.
12 .	65	64	62	$\frac{1}{40}$ do.
1 P.M. .	64	64	62·5	$\frac{1}{80}$ do.
2 .	65	64	63·5	$\frac{1}{80}$ do.
3 .	65	64	63	Stationary.
4 .	65·5	64	63	Do.
5 .	63·5	63·5	62·5	Do.
6 .	62	63	62	$\frac{1}{40}$ falling.
				For 7½° F. $\frac{13}{16}$ of an inch; that is, $\frac{1}{47}$th of an inch for 1° F.

IV.

WEDNESDAY, SEPTEMBER 23rd. Still Morning—A light Breeze.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 5 A.M. .	50°	51°	0°	
6 .	51	52	53	Stationary.
7 .	55	54	54	Just beginning to rise.
8 .	58	57	55·25	$\frac{1}{40}$ rise.
9 .	60	59	57	$\frac{3}{80}$ do.
10 .	60	60	58	$\frac{1}{40}$ do.
11 .	61	61	58·5	$\frac{1}{80}$ do.
12 .	64	63·5	60·5	$\frac{3}{80}$ do.
1 P.M. .	64	64	61	$\frac{1}{40}$ do.
2 .	65	65	63	$\frac{3}{40}$ do.
3 .	65	65	64	$\frac{1}{40}$ do.
4 .	65	64·5	64	Stationary.
5 .	63·5	64·5	64	Do.
				Total rise for 11° F. $\frac{23}{60}$ of an inch; that is, $\frac{1}{30}$th of an inch for 1° F.

V.

THURSDAY, SEPTEMBER 24th. Mild, still Morning.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 6 A.M. .	55°	55°	0°	
7 .	55	56	55	Stationary.
8 .	58	57	56	Do.
9 .	58	58	57	Do.
10 .	61	60	58	$\frac{1}{40}$ rise.
11 .	62	61	60	$\frac{2}{40}$ do.
12 .	66	63	60·5	$\frac{3}{40}$ do.
1 P.M. .	66	63	61	$\frac{1}{40}$ do.
2 .	65	63·5	62	$\frac{1}{40}$ do.
3 .	63	63	62	$\frac{1}{40}$ do.
4 .	61·5	62	62	Stationary.
5 .	61	61·5	61	Do.
6 .	59	60·5	61	Do.
				Total rise for 6° F., $\frac{13}{40}$ of an inch; that is, $\frac{1}{31}$th of an inch for 1° F.

VI.

FRIDAY, SEPTEMBER 25th. Mild, still Morning.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 6 A.M. .	53	54°	0°	
7 .	54	55·5	55·5	Stationary.
8 .	54	56	56	Do.
9 .	58	57·5	57	Do.
10 .	58	57·5	57·5	Do.
11 .	58·5	58	58	$\frac{1}{40}$ rise.
12 .	60	59	58	$\frac{1}{40}$ do.
1 P.M. .	61	59	58·5	$\frac{1}{40}$ do.
2 .	64	61	60·5	$\frac{3}{40}$ do.
3 .	63	62	60	$\frac{1}{40}$ do.
4 .	62	62	60	$\frac{1}{40}$ do.
5 .	62·5	60·5	60	0 do.
6 .	61	60	59	Stationary.
				Total rise for 4·5° F., $\frac{13}{40}$ of an inch; that is, $\frac{1}{35}$th of an inch for 1° F.

VII.

SATURDAY, SEPTEMBER 26th. Morning Wet and Stormy.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 6 A M. .	59°	57°	0°	
11 .	59	57	55	$\frac{1}{80}$ rise.
12 .	60	59	56	$\frac{3}{40}$ do.
1 P M. .	62	60	56	0 do.
2 .	63	61	57	$\frac{3}{80}$ do.
3 .	63	60	58	$\frac{1}{80}$ do.
4 .	62	62·5	58	0 do.
5 .	61	60	58	0 do.
6 .	60	59	58	0 do.
became dark	..	..	..	Just falling.
				Total rise for 3° F. $\frac{3}{80}$ of an inch; that is, $\frac{1}{80}$th of an inch for 1° F.
				The outer Ribs were also observed, and gave the same result within a very small quantity.

VIII.

SUNDAY, SEPTEMBER 27th. A fine, mild Morning—the Wind southerly.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 7 A.M. .	54°	..	..	
2 P.M. .	62	..	..	$\frac{7}{40}$ rise.
				Total rise for 8° F. $\frac{7}{40}$ of an inch; that is $\frac{1}{45}$th of an inch for 1° F.

IX.

MONDAY, SEPTEMBER 28th. Fine, mild Morning.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 6 A.M. .	60°	60°	59·5°	
7 .	60·5	60·5	59	Stationary.
8 .	62	61	59	$\frac{1}{50}$ rise.
9 .	64	63	61	$\frac{2}{40}$ do.
10 .	66	65	64	0 do.
11 .	69	67	63	$\frac{2}{40}$ do.
12 .	72	69	64	$\frac{2}{40}$ do.
1 P.M. .	74	69	66	$\frac{2}{40}$ do.
2 .	73	70	67	$\frac{1}{40}$ do.
3 .	73	70	68	$\frac{1}{50}$ do.
4 .	69·5	68	67·5	$\frac{1}{50}$ do.
5 .	68	67·5	67	Stationary.
6 .	67·5	67	67	Do.
				Total rise for 7·5 F. $\frac{11}{50}$ of an inch; that is, $\frac{1}{35}$th of an inch for 1° F.

When these experiments were tried nearly all the road-plates were on, but none of the cornice, frieze, or parapet.

RESULT OF EXPERIMENTS.

The result of the preceding experiments being collected together, we have the following Table:—

No. of Experiment.	Total Variation in Temperature, in degrees F.	Total Rise in Arch, in parts of an inch.	Rise for 1° F.
I.	15°	$\frac{34}{50}$	$\frac{1}{48}$
II.	10	$\frac{16}{50}$	$\frac{1}{50}$
III.	7·5	$\frac{13}{50}$	$\frac{1}{47}$
IV.	11	$\frac{28}{50}$	$\frac{1}{40}$
V.	6	$\frac{18}{50}$	$\frac{1}{27}$
VI.	4·5	$\frac{10}{50}$	$\frac{1}{26}$
VII.	3	$\frac{9}{50}$	$\frac{1}{28}$
VIII.	8	$\frac{4}{50}$	$\frac{1}{40}$
IX.	7·5	$\frac{11}{50}$	$\frac{1}{28}$

The discrepancies in these results are not greater than is to be expected in observations of this nature. If we take the mean of the whole, the rise will be $\frac{2 \cdot 1}{80}$, or $\frac{1}{40}$th of an inch, for each degree of temperature. Hence the total rise for 50° F. would be, according to the result of these observations, 1·25 inches.

CALCULATION FOR THE RISE OF THE ARCH OF SOUTHWARK BRIDGE ON AN INCREASE OF 50° F. IN TEMPERATURE.*

Length of chord of extrados . . . . . 246 feet, or 2952 inches.
Versed sine, or rise of extrados . . . . 23 feet 1 inch, or 277 inches.

Diameter of circle of extrados $= \frac{1476 \times 1476}{277} + 277 = 8141 \cdot 89$ inches.

Then 4070·945 : 1476 :: radius : sin $\frac{1}{2}$ arch,

$$\begin{array}{r} 10 \cdot 0000000 \\ 3 \cdot 1690864 \\ \hline 13 \cdot 1690864 \\ 3 \cdot 6096904 \\ \hline 9 \cdot 5593960 \\ \hline \end{array}$$

Whence $\frac{1}{2}$ arch = 21° 31′ 30″,
And circumference of circle of arch = 3·1416 × 8141·89 = 25578·56 inches,
Therefore 360° : 21° 15′ 30″ :: 25578·56 : Length of $\frac{1}{2}$ arch (= 1510·4),
or length of arch = 3020·8 inches.

Taking the expansion of iron according to Lavoisier for 1° F. at ·00000618 its length, we have for 50° F.,

the expansion of arch = 3020·8 × 50 × ·00000618
= ·9334272
∴ length of expanded arch = 3021·7334272

The height of the arch of which the chord is 2952 inches, and the length 3021·7334272, would be 278·9 inches.

* This calculation must be considered as an approximation only. It proceeds on the hypothesis that the arch, when expanded, will be a longer portion of the circle of the same radius. The curve will depend on the pressure which exists at every point, and consequently its exact form cannot be assigned.

Then 278·9 inches − 277 inches = 1·9, or 2 inches nearly, which would be the rise of the arch consequent on 50° temperature.

Rise by calculation	1·9
Do. by observation	1·25
Difference between calculation and observation .	·75, or $\frac{3}{4}$ of an inch.

On comparing the mean of the experiments with the preceding calculation we find a considerable difference. The mean of the experiments on the rise of the centre arch gives a rise of $\frac{1}{4}$ of an inch for every 10 degrees of temperature; so that, taking the mean rise for every 10 degrees at $\frac{1}{4}$ of an inch, and the total range at 50°, we have a total rise of $1\frac{1}{4}$ inch for the centre arch, which is $\frac{3}{4}$ of an inch less rise than that given by preceding calculation. Considering, however, the frequent variations of temperature in our climate during the day, and the great mass of metal* exposed to its influence, the difference is not surprising; indeed it was found that the arches did not attain their maximum expansion or contraction until several hours after the maximum or minimum temperatures.

* The weight of the centre arch is 1665 tons, and of each of the side arches 1460; total, 4585 tons.

ON THE EXPANSION OF IRON UNDER THE ATMOSPHERIC VARIATIONS OF TEMPERATURE.

In the year 1818, being desirous of ascertaining the effect of temperature on the strength of iron, I suspended a bar of wrought iron, $\frac{1}{4}$ of an inch in thickness, in such a position that a lever attached to it should indicate, by the expansion of the iron rod, the variations of temperature on a graduated arc. The end of the lever was then loaded with a weight equivalent to within the limit of the average tension of a rod of similar dimensions, and the lever being placed exactly level, on a zero point, the variations of temperature were exhibited on either side of zero. The experiment commenced in October, from which period, as the weather became warmer or colder, the lever continued to rise and fall, morning, noon, and night, until a frost in November came on, when the bar was found broken. The conclusion was, that the iron had become weakened by the cold.

As a further example of the effect of expansion, but arising from a different cause, I may relate the following.

During the severe frost which took place in the winter of 1820, several of the lamp-posts, placed on the pedestals of the Southwark Bridge, were found broken at the tenons, or joggles, which fit into corresponding holes, or mortices, cast in the pedestals; several lamp-posts also were thrown out of their perpendicular position. On lifting off the lamp-posts, it was found that the holes were filled with ice, occasioned by the water lodging in them, and which, having been frozen, and consequently expanded, displaced the lamp-posts; and, in several instances, owing to the unequal depths of the holes, broke them at the tenons. To prevent a similar recurrence of these accidents, counter holes were drilled into the bottom of each hole laterally, by which the water now runs off, and nothing of the kind has since occurred.

A set of experiments was made at the request of Mr. Rennie, by Mr. Samuel Walker, of Rotherham, in the months of July and August, 1818, on the length of a large portion of the frieze-plates of the bridge at various temperatures. The frieze-plates, $2\frac{1}{2}$ inches thick, and 2 feet 10 inches high, were bolted together and raised upon a firm platform 3 feet from the ground. The length of the plates bolted together was 231 feet $\frac{1}{2}$ inch, at a temperature of 60° F. The temperature of the atmosphere and of the plates was noted at 5 o'clock in the morning and 3 o'clock in the afternoon, the coolest and hottest portions of the day; thermometers for this purpose being suspended in the air and laid on the plates. The results are given in the following Table.

TABLE showing the EXPANSION and CONTRACTION of the FRIEZE PLATES of SOUTHWARK BRIDGE.

JULY, 1818.	Therm. 5 A.M.	Therm. 3 P.M. in the Shade.	Therm. 3 P.M. in the open Air.	Therm. 5 A.M. laid on the Plates.	Therm. 3 P.M. laid on the Plates.	Length of the Plates, 5 A.M. Feet.	Length of the Plates, 5 A.M. In.	Length of the Plates, 3 P.M. Feet.	Length of the Plates, 3 P.M. In.	REMARKS.
17th	60°	76°	73°	62°	83°	231	1	231	$1\frac{5}{8}$	Weather very sultry—rain just begun—thermometer exposed to the rain.
18th	60	76	84	59	93	231	$0\frac{1}{8}$	231	$1\frac{1}{8}$	Weather at 3 P.M.—hot, but rather cloudy.
20th	70	70	$71\frac{1}{8}$	68	78	231	$0\frac{3}{4}$	231	$1\frac{7}{32}$	Weather at 3 P.M.—cloudy.
21st	63	74	79	60	84	231	$0\frac{1}{8}$	231	$1\frac{1}{8}$	Rather cloudy—occasional sun.
22nd	63	75	79	63	87	231	$0\frac{1}{8}$	231	$1\frac{3}{16}$	Sunshine, and rather windy.
23rd	68	81	96	68	99	231	$0\frac{3}{4}$	231	$1\frac{9}{16}$	Clear sky, and rather hot.
24th	68	87	98	68	104	231	$0\frac{3}{4}$	231	$1\frac{7}{8}$	Clear sky, and brisk wind—very hot.
25th	64	82	96	65	95*	231	$0\frac{7}{8}$	231	$1\frac{13}{16}$	The day had been bright till 3 P.M., when it suddenly became windy, and rather cloudy or hazy.
27th	64	67	61	63	62	231	$0\frac{5}{8}$	231	$0\frac{11}{16}$	Cloudy, wet, and no sun.
28th	53	68	73	50	85	231	$0\frac{1}{8}$	231	$1\frac{1}{4}$	Sun shone, but cloudy.
29th	61	81	82	61	89	231	$0\frac{7}{16}$	231	1	Brisk wind, a little sun, and rather cloudy.
30th	66	$73\frac{3}{4}$	79	65	82	231	$0\frac{1}{8}$	231	$0\frac{7}{8}$	A little wind, and cloudy.

* The Thermometer, when laid on the plate, and concealed from the sun, did not vary. Being applied underneath the plate, it stood at 92°.

AUGUST, 1818.	Therm. at 5 o'clock.	Therm. at 2 o'clock, in the Shade.	Therm. at 2 o'clock, in the Sun.	Therm. at 7 o'clock.	Therm. laid on Plates at 5 o'clock.	Therm. on Plates at 2 o'clock.	Therm. on Plates at 7 o'clock.	Length of 17 Plates at 5 o'clock. Feet.	Length of 17 Plates at 5 o'clock. In.	Length of 17 Plates at 2 o'clock. Feet.	Length of 17 Plates at 2 o'clock. In.	Length of 17 Plates at 7 o'clock. Feet.	Length of 17 Plates at 7 o'clock. In.	REMARKS.
5th	60°	79°	108°	81°	58°	113°	89°	231	$0\frac{9}{16}$	231	$1\frac{10}{16}$	231	$1\frac{5}{8}$	Very hot, and a clear sky.
6th	66	$71\frac{1}{8}$	74	66	67	81	65	231	$0\frac{5}{8}$	231	$1\frac{3}{8}$	231	$1\frac{1}{8}$	Cloudy, and a brisk wind.
7th	53	65	77	66	51	81	65	231	$0\frac{1}{4}$	231	$1\frac{7}{16}$	231	$1\frac{3}{16}$	Clear sun, and a cool brisk wind.
8th	58	64	67	64	55	68	64	231	$0\frac{3}{8}$	231	$0\frac{13}{16}$	231	$0\frac{9}{16}$	Cloudy, and a little wet.

ON THE EXPANSION OF STONE BRIDGES.

In the year 1834, two years after the completion of the stone bridge over the Thames, at Staines, and after the arches had attained their full settlement, openings were observed in the joints of the parapets immediately over the springing of the arches, and a distortion, or sinking of the upper curve of the parapets, was the result. Convinced of the true cause, I inserted into each of the openings an iron wedge until it descended to an inch and a half, and with a tracer marked the lowest point of descent of each wedge. This was in the month of January, when the weather was very cold. The same wedges were carefully inserted in the openings, once a week, until the month of May, when the wedges could no longer enter, as the joints had closed so firmly, as, in some instances, to chip off small pieces of stone from the upper edges of the parapet. The reverse effect, however, was found to take place with the joints immediately over the crowns of the arches, which, from being quite close before, were then open. The conclusion was, that the arches had been affected by variations of temperature: contracting, and, consequently, descending in the winter, and opening the spandril joints, and expanding and closing these joints in the summer; thus the joints of the parapets (which were made of single slabs of granite, for the whole height) became good indicators of the changes of temperature. These phenomena had often been noticed by me in the Waterloo and other bridges, where, from the joints having been made good with Roman cement in the winter, were found broken, or rather crushed, in the summer. No doubt therefore remained of the fact, although it was desirable to verify it by experiment. Accordingly, I procured several samples of granite, sandstone, and slate, as nearly as possible of similar dimensions, and, having placed them in a properly constructed oven, ascertained their rates of expansion, by scales and micrometer screws. The specimens were measured as they received a fresh increase of heat, and, when taken out of the oven, were placed in a wooden box, and allowed to cool gradually, so that the decrease of heat might be measured in the same manner. The results will be found in the annexed Table.

From the preceding statements, we may conclude that all buildings are subject to expansion; but that the range of temperature, arising from solar heat, cannot be prejudicial to their stability.

EXPERIMENTS ON THE EXPANSION OF IRON AND STONE.

By GEORGE RENNIE, Esq.

March and April, 1834.

MATERIAL.	Length in Feet.	Breadth; parts of a Foot.	Thickness; parts of a Foot.	Contents; parts of a Foot.	Temperature, F., in Air.	Temperature, F., when Heated.	Expansion in Total Length; Feet.	Rate; the Length in Air being 1.
Cast Iron . . .	4·886	·416	·175	·356	32°	70°	Not perceptible	..
Do.	..	..	..	..	..	86	·003472	1·0007104
Do.	..	..	..	..	..	200	·008333	1·0017049
Do.	..	..	..	..	..	100	·003472	1·0007104
Do.	..	..	..	..	..	150	·0052083	1·001065
Granite	5·	·5	·3	·833	45	220	·01458	1·002916
Do.	..	..	..	..	..	100	·00208	1·000416
Clay Slate . . .	5·00625	·75	·2083	·791	46	70	..	..
Do.	..	..	..	..	..	87	·00208	1·000416
Do.	..	..	..	..	..	104	·00347	1·000693
York Paving . .	4·916	·416	·25	·512	46	95	·00833	1·001695
Micacious Sandstone	..	..	..	..	..	98	·00333	1·000678
Do.	5·166	·5	·25	·646	46	70	..	..
Do.	..	..	..	..	..	98	·00694	1·001344
Do.	6·	·6	·25	1·	52	200	·010416	1·001736
Do.	..	..	..	..	45	180	·00625	1·001041
Do.	..	..	..	..	52	200	·00625	1·001041
Do.	..	..	..	..	..	150	·00416	1·000832
Do.	..	..	..	..	..	100	·003125	1·000520
Do.	5·	·5	·25	·625	52	122	·00416	1·000832
Do.	..	..	..	..	..	140	·00333	1·000666
Do.	..	..	..	..	45	100	002083	1·000416
Do.	..	..	..	..	..	160	·00625	1·00125
Do.	5·	·416	·25	·521	45	260	·00729	1·001458
Stock Brick . .	..	..	..	..	52	260	..	1·0025

CALCULATIONS OF THE EFFECT OF 50° INCREASE OF TEMPERATURE ON THE MIDDLE ARCH OF STAINES BRIDGE.

	Feet.	In.
Chord of extrados	82	3, or 987 inches.
Versed sine	9	4, or 112 inches.

$$\text{The diameter} = \frac{493\cdot5 \times 493\cdot5}{112} + 112 = 2886\cdot48 \text{ inches,}$$

$$\text{and radius} = \frac{2286\cdot48}{2} = 1143\cdot24$$

Then $1143 \cdot 24 : 493 \cdot 5 :: \text{rad} : \sin \frac{1}{2} \text{ arc}$

$$\begin{array}{r} 10 \cdot 000000 \\ 2 \cdot 693287 \\ \hline 12 \cdot 693287 \\ 3 \cdot 058137 \\ \hline 9 \cdot 635150 \\ \hline \end{array}$$

and $\frac{1}{2}$ arch $= 25° \; 34' \; 409''$.

The semicircumference $= 1143 \cdot 24 \times 3 \cdot 1416 = 3591 \cdot 602784$

$180° : 25° \; 34' \cdot 409 :: 3591 \cdot 6 :$ length of $\frac{1}{2}$ arc

$10800 : \quad 1534 \cdot 4 \quad :: 3591 \cdot 6 : 510 \cdot 27.$

Length of whole arch $= 2 \times 510 \cdot 27 = 1020 \cdot 54$

Amount of expansion $= 1020 \cdot 54 \times \cdot 00000438 \times 50° = 0 \cdot 22349826$

Length of expanded arch $= 1020 \cdot 54 + 0 \cdot 22349826 = 1020 \cdot 76$ inches, &c.

The height or versed sine of an arc, of which the chord is 987 inches, and the length 1020·76 inches, would be 112·3 inches.

$112 \cdot 3 - 112 = 0 \cdot 3$, which is the rise consequent on 50° increase of temperature.

From the extremity of chord to top of parapet is 176 inches, and $493 \cdot 5 : 176 :: 0 \cdot 3 : 0 \cdot 10$ inch, which is the effect of this rise in a lateral direction at the level of the parapet over the centre of each pier.

$$\text{Chord of } \tfrac{1}{2} \text{ arch} = \sqrt[2]{(112)^2 + (493 \cdot 5)^2} = 506 \cdot 0 \text{ inches.}$$

$$\text{Chord of expanded } \tfrac{1}{2} \text{ arch} = \sqrt[2]{(506 \cdot 0)^2 - (112 \cdot 3)^2} = 493 \cdot 38 \text{ inches.}$$

The lateral movement of one spandril, from the crown of the arch, by the rise,

$$= 493 \cdot 5 - 493 \cdot 38 = 0 \cdot 12 \text{ inches.}$$

The whole lateral movement from the crown of the arch by the rise, each spandril receding the same distance from the middle point,

$$= 2 \times 0 \cdot 12 = 0 \cdot 24.$$

In addition to the above lateral movement, there would be a departure of each spandril from the perpendicular to the crown of the arch, equivalent to

what would be produced by a movement round the point at the crown by a fall of 0·3 at the distance of $\frac{1}{2}$ the chord of the arc. Height from crown to parapet-level is 66 inches.

Then 493·5 : 66 :: 0·3 : 0·04. The departure of one spandril from the perpendicular.

The whole separation of spandrils at parapet-level over the crown of arch due to this revolving motion,

$$= 2 \times 0 \cdot 04 = 0 \cdot 08.$$

Total opening at the level of parapet over the crown,

$$= 0 \cdot 24 \times 0 \cdot 08 = 0 \cdot 32.$$

The expansion of *one* spandril (that is, between centre of arch and centre of pier) would be $= 493 \cdot 5 \times 50° \times .00000438 = 0 \cdot 108$ inches.

This expansion would tend to diminish the opening already mentioned as taking place above the crown, and would tend to increase the compression which takes place above the piers.*

The following are the results of the experiments of M. Destigny on the expansion of stone and metals.

ON THE EXPANSION OF STONE, AND THE MEANS OF MEASURING IT. By M. DESTIGNY.†

The following were the results at 40° Reaumur, or 122° Fahrenheit :—

The expansion of Iron being	196
That of clear white Carrara Marble, second quality .	136
That of French Marble of Sost	91·10
That of Marble of St. Béat	67
That of St. Leu Stone	104
That of Stone of Vernon-sur-Seine	68·95

* The multiplier ·00000438 for the expansion of granite, is taken from the experiments on the expansion of stone by Alexander J. Adie, recorded in the "Transactions of the Royal Society of Edinburgh," Vol. XIII.

† See "Bulletin de la Societé d'Encouragement;" also, see Vol. X. of "Repertory of Arts," page 376.

TABLE of the absolute EXPANSION of the different STONEY SUBSTANCES, as well as of BRASS and IRON, for a variation of Temperature of 100° Centigrade, or 212° Reaumur.

SUBSTANCES.	Absolute Expansion.	Expansion for the Length of one Metre.
Brass	0·00187821	1·8782
Soft Wrought Iron . .	0·00122045	1·2200
Carrara Marble . . .	0·00084867	0·8487
St. Béat Marble . . .	0·00041810	0·4181
Sost Marble	0·00056849	0·5685
Vernon Stone	0·00043027	0·4303
St. Leu Stone	0·00064890	0·6490

M. Destigny found no difference in the expansion of the stone, whether dry or moist.

VII.—*Description of the State of the Suspension Bridge at Montrose, after it had been rendered impassable by the Hurricane of the 11th of October, 1838; with Remarks on the Construction of that and other Suspension Bridges, in reference to the action of violent Gales of Wind.*

By C. W. PASLEY, C.B., Colonel Royal Engineers, F.R.S., Hon. M. Inst. C. E., &c.

In the afternoon and night of the 11th of October, 1838, I travelled from Perth to Inverness by the mail, in extremely boisterous weather, which I afterwards learned had nearly approached to a hurricane, having torn up a great number of trees, swept away the produce of whole fields of corn, and otherwise caused extensive damage to property over the whole of the North of Scotland. From Inverness I went to Aberdeen, whence it was my original intention to proceed direct to Dundee; but having heard that the same storm had destroyed part of the suspension bridge of Montrose, I was induced to stop there, that I might have an opportunity of inspecting the construction of that bridge, and of ascertaining from what cause, or, at least, in what part, it had given way; having always been of opinion that from the example of failures some of the most instructive lessons in practical architecture or engineering are to be derived. I found on inspection that about one-third part of the roadway of the bridge had been entirely carried away, excepting a sort of external skirting-board or ornamental fascia, which remained in a shattered state on the west side of the bridge, hanging by the suspension rods, which were not entirely broken, but only injured and bent on that side; the chains, however, four in number, which extended in two parallel lines of two tiers each, remained apparently quite perfect. The length of roadway between the two piers of masonry which supported the chains appeared to be about 410 feet, and over this space the links of each chain consisted of six iron bars, each 1 inch thick and 5 inches deep, and the connecting plates of seven pieces of the same width but shorter and deeper in the usual proportion.

I was informed at Montrose that the whole of the chains were originally constructed with four bars only, but that in consequence of one of them giving way soon after the bridge was finished, two more bars had been added to each, in the central part only. The cause of this accident was said to have been a boat race, at which a multitude of spectators suddenly rushing from one side

of the bridge to the other broke one of the upper chains, and thereby caused the loss of some lives. The failure having proved the insufficiency of the chains in their existing state, two plans were proposed for increasing their strength, the one by adding a third chain similar to the former over each side of the bridge so as to make six chains in all, in two tiers of three each; the other plan was to increase the strength of the existing chains by adding two more iron bars or plates to all the parts of each, and connecting them by new bolts longer in proportion. This plan (suggested by Mr. Rendel) was adopted and executed under his direction in the central part of the bridge, but not at the extreme ends of it, where it was unnecessary, as the roadway did not depend upon them, being there formed of solid masonry. The efficiency of this judicious arrangement has since been sufficiently proved by the circumstance of all the four chains remaining uninjured during the partial destruction of the roadway.

The guard of the mail coach and other persons who passed over the bridge just before it gave way, stated that they felt the bridge vibrate in the most violent manner, and that they had always experienced the same sort of vibration in a greater or lesser degree during every strong gale of wind. In Mr. Provis's account of the Menai bridge, he states that no transverse vibration of the roadway ever took place during the most violent storms, but that the whole of it undulated longitudinally like the waves of the sea, which is corroborated by my own observation of the action of a strong gale of wind upon a light suspension bridge for foot passengers over the Tweed at Melrose; in which, though about 300 feet long and only six feet wide, not the smallest transverse vibration or lateral movement was perceptible, although the wind blew broadside on, but a considerable undulation was going on continually throughout its whole length. I therefore consider, that the persons who described the suspension bridge at Montrose as having been in a state of vibration, must either have confounded that term with undulation, which is sometimes done, or erroneously ascribed its unsteadiness when they passed over it, to a motion of the former instead of the latter sort.

On my return to Chatham by way of London, I inspected the suspension bridge at Hammersmith on the 30th of October, two days after a very violent storm that had done a great deal of injury in that town and neighbourhood, and blown down two walls and part of the roof of a new building then in progress in the brewery of Sir F. Booth at Brentford. About a month afterwards, on a very strong gale of wind setting in, I went to inspect the same bridge a second

time, when I found no undulation going on; and having made inquiry of persons who were in the habit of passing over it in all weathers, I ascertained that this bridge has never been affected in that alarming manner.

When I compared the construction of the roadway of this bridge with that of Montrose, I became convinced of the correctness of an opinion I had previously formed, that all injuries that have ever occurred to the roadways of suspension bridges must have been caused by the violent action of the wind from below, such as I had an opportunity of observing at Chatham, when looking down upon the Royal Dock Yard at that place in a tremendous hurricane, on the 29th of November, 1836, when the wind acting from below on the far side of one of the roofs of a large ship-building shed, caused it to rise into view from time to time, for when it was in its proper position, I could not see it at all. Thus it continued for more than an hour flapping up and down, and adhering to the other side of the same roof by the ridge only, in the same manner that a person may move the cover of a book with his hand; until at last I saw a large fragment of it carried away, which, as I afterwards ascertained, measured about 40 feet by 50, and which, after floating a little while in the air, as a sheet of paper would in a light wind, fell to the ground at the distance of 50 yards, breaking through part of the roof of a storehouse. If the planking and purlins of this roof had been bolted down to the principal rafters, and secured from below, this accident would not have happened. On that same day, the hurricane overset a stage coach on Rochester bridge, and blew away part of the stone balustrade of that bridge; and also destroyed part of the roadway of the suspension chain pier at Brighton, and I have no doubt of its having done so by acting upon it from below. That this is the usual action of the wind in hurricanes was sufficiently proved by the fate of the roof in Chatham Dock Yard; and supposing it to act thus on a suspension bridge, it must first force the roadway of the bridge upwards, until its own weight, or a temporary lull of the violence of the wind, may cause it to fall down again to the full length of the suspension rods, in continual undulations, the rods being thereby alternately subject to compression and extension must give way at last, under the same process which a person naturally adopts when he wishes to break a piece of wire or light iron; and in the course of this process, the chains also being at one time relieved from part of the weight of the roadway, and afterwards having it thrown upon them by sudden jerks, must be put in motion. Such, I repeat, must have been the sort of action, by which a consider-

able portion of the roadway of the chain pier at Brighton, and also of the suspension bridge at Montrose, were destroyed at the periods above mentioned.

The reason of the superior safety of the suspension bridge at Hammersmith became evident to me, on comparing the construction of the roadway of that bridge with those of Brighton and Montrose. In the latter there were only light railings on each side of the bridge, composed chiefly of slight iron bars, which, in the chain pier at Brighton, were all vertical; but in that of Montrose they intersected each other diagonally; but in neither of those sort of parapets was there such stiffness in a vertical direction, as to prevent the undulation of the roadway in a violent gale of wind or hurricane, although they might check it in a slight degree during a moderate breeze. It was, however, observed in the case of the chain pier at Brighton, the partial destruction of which was witnessed by numerous spectators, that no part of the roadway gave way in the hurricane of November, 1836, until the side railings were previously shattered to pieces and blown away, after which the undulations became perceptibly more violent, and ended in carrying away the roadway also. In the suspension bridge at Hammersmith, on the contrary, there are four lines of strong trussing along the whole length of the roadway from one end of it to the other, so firmly connected by iron bolts and plates to the roadway bearers below, which consist of double joists of wood, that it is quite impossible for the strongest gale of wind to produce any undulations in the roadway, without breaking all the parts of these trusses to pieces, which are far too strong to be thus acted upon, being composed partly of cast-iron, and partly of substantial wood-work. The side railing of the Plate. Fig. 3. Hammersmith bridge is formed by hollow cast-iron pillars about $4\frac{1}{2}$ feet high, at intervals of about 15 feet apart, having a longitudinal sill below and a cap or top rail above, with two braces intersecting each other diagonally in the form of the letter X, all of these last-mentioned parts being of wood-work, and of considerable scantling. In addition to these there are two other lines of trussing, consisting of a series of king-posts, each formed by a hollow cast-iron pillar, similar to those before described, and a couple of wooden braces in the form of rafters, these pillars being at intervals of about 25 feet apart, the whole resting on a strong longitudinal sill, and each pair of small rafters abutting at top against the heads of those cast-iron pillars, and at bottom against cast-iron blocks. These lines of king-post trussing being immediately under two of the four lines of chains, by which the roadway is supported, so as to divide the carriage-way, which is in the centre of the bridge, from the footpaths on each

side of it, are reduced in height in the middle length of the bridge, where those two lines of chains come down close to the roadway. The two other lines of chains are over the sides of the bridge, and the vertical rods suspended from them pass close to the trussed side railings before described; and thus, as there are two tiers of chains in each line, there are eight chains in all, the bars of which are all 1 inch in width by 5 inches in depth; but while there are six bars in each link of the two centre lines of chains, there are only four bars in the others, which support the outer sides of the foot paths.

To illustrate these particulars, I have annexed a sketch of the suspension bridge of Montrose in elevation, as it appeared when I inspected it five days after it had been rendered impassable (*see* Fig. 1). In this sketch I have omitted the useless fragment of skirting-board or fascia before mentioned, on one side of the bridge, which would have rendered the figure confused. I did not pay much attention to the precise construction of the side railing at that time, and therefore the sketch of it given in Fig. 2 is from memory; but it was certainly in no respect stronger than that shown in the sketch. I have also annexed sketches in elevation of the side railing and king-post trusses of the suspension bridge of Hammersmith (*see* Figs. 3 and 4).

On an attentive perusal of Mr. Provis's account of the Menai bridge, I am sorry to say that it does not appear to me to be so secure, against the effects which may be apprehended, from the violent undulations to which it has always been subject, as could be desired. By this observation I mean no disparagement to my honoured friend, that most excellent man and distinguished engineer, the late President of this Institution, by whom the Menai bridge was designed, and under whose directions it was executed, because engineers had then no experience of a suspension bridge of so very large a span; and in constructions entirely and absolutely new, no man, however great his talents may be, can be expected to foresee every contingency that ought to be provided for. I believe that Captain Brown's suspension bridge over the Tweed, at Norham, near Berwick, said to be about 360 feet in length between the points of support, was not only the largest, but the most ingenious and judicious bridge of this sort that had ever been constructed, until Mr. Telford, by authority of the Government, undertook the gigantic task of constructing a bridge over the Menai Strait, at such a height as to admit the largest sailing vessels to pass without striking their top-masts, by the successful execution of which, he not only exceeded all former suspension bridges in magnitude, but

made, as I conceive, a great improvement, by adopting rectangular iron bars 1 inch wide, and more than 3 inches deep, for the links of his chains, and giving them altogether a much more perfect form than those of Captain Brown, who used round iron, both for the parts of his chains and for his suspension rods, not only in his bridge over the Tweed, but also in his chain pier at Brighton. Before the Menai bridge was quite finished, a very violent gale of wind broke some of the suspension rods, and also a few of the roadway bearers which were of wrought iron; and as the resident engineers reported to Mr. Telford, it caused not only a very great and continual undulation of the roadway, but made the chains, 16 in number, which were arranged in four lines of four tiers of chains each, to vibrate, and even slightly altered the position of one of them. It appears, however, that the persons who witnessed these movements, must have considered the lateral motion of the chains as the most important, and as being the cause of the undulations in the roadway, for immediate precautions were taken to counteract the former, by introducing two parallel rows of cast-iron tubes about four inches in diameter, transversely, which connected and stiffened all the chains of the upper tier, and all those of the lower tier but one, and were themselves connected vertically by a sort of net-work, of light wrought-iron rods, intersecting each other diagonally, as represented in Fig. 6. Mr. Provis states that after this addition, the undulations of the roadway in violent storms of wind were reduced to about one-half of their former height. But we know that they have not been entirely prevented, for I have been informed by several gentlemen, who have inquired minutely into the state of the Menai bridge during strong gales of wind, that the undulations are then always very violent. Mr. Rickman, junior, has stated that he was informed by the toll-keeper, that the difference of level in very violent gales was not less than 6 feet, from the roadway rising and falling 3 feet above, and as much below, its proper and usual level. From this circumstance, contrasted with the stability of the Hammersmith bridge, in all weathers, we may now infer, although it was not foreseen at the time, that it would have been better to truss the roadway of the Menai bridge, on the principle afterwards adopted by Mr. W. Tierney Clark, the engineer of the Hammersmith bridge, than to stiffen the chains by the iron tubes and diagonal lacing before described. In fact, the roadway, from its very great surface, presents such an immense obstruction to the fury of a hurricane acting upon it from below, that if it be prevented from undulating, it can scarcely be supposed

that the utmost force of the wind could move the chains at all, having comparatively so very little surface to oppose to it, and which must be held down by the great weight of the roadway, so long as *that* remains at rest. Let it be understood, that I do not consider it any discredit to the very able resident engineers and skilful workmen employed at the Menai bridge, when the first storm occurred, in stating my opinion, that they made a mistake in recommending Mr. Telford to stiffen the chains, instead of the roadway, because they had not the experience which we possess in the present day.

After I had inspected the suspension bridge at Hammersmith, I called upon Mr. Clark, who informed me that before he decided definitively upon all the details of the construction of that bridge, he made a model of it on a very large scale, which he exposed to all weathers, and which must have given him more insight into the action of the wind on a suspension bridge, than theory or reflection alone could have done; besides which, he had the experience of the undulations of the Menai bridge, as a result to be guarded against. Mr. Clark showed me his designs for several new suspension bridges, one of which is to be thrown over the River St. John's in New Brunswick, in which he proposes to use a simpler mode of trussing than that adopted by him for the bridge at Hammersmith, which I consider an improvement. I also called upon Mr. Rendel, on learning that he was again employed to direct the repairs of the bridge of Montrose, in consequence of the late hurricane, and was pleased to find that his opinion, as to the destructive action of the wind on the roadway from below, agreed with mine; and indeed he assured me that, in an official letter written previously to this event, he had recorded his opinion, that he considered the roadway to be the weakest part of that structure. Mr. Rendel was also good enough to show me a design for trussing the roadway of a new suspension bridge, different from Mr. Clark's, but which could not fail to be extremely efficient. As both these gentlemen are members of this Institution, I shall not presume to anticipate any thing further that they may be pleased to communicate; but as my ideas on the peculiar action of violent gales of wind upon suspension bridges were derived from my own observation, before I communicated with either of them, and as the mode of preventing it adopted by Mr. Clark in the suspension bridge at Hammersmith, has been proved to be effectual, it gives me pleasure in having an opportunity of ascribing to that engineer, the merit which he deserves, of having been the first to make this important improvement in the construction of suspension bridges.

The only printed paper in which I have seen the necessity of a stiff trussed railing for the sides of a suspension bridge alluded to, is one by my brother officer, Lieut.-Colonel Reid of the Royal Engineers, recently appointed Governor of the Bermudas, and the author of a very important Essay on the Law of Storms, who after having observed the destructive effects of a tremendous hurricane that took place at Barbadoes, immediately before his arrival there, which naturally drew his attention to this subject, was afterwards an eye-witness of the destruction of part of the roadway of the chain pier at Brighton, in November 1836; and as the very same part of that pier had been destroyed on a former occasion at night, by a violent storm of wind, accompanied by thunder and lightning, he states his opinion, that the wind also had been the cause of this previous failure, and not the lightning, as had generally been supposed, and he remarks on the iron rods of the side railing of the chain pier being all arranged vertically without any diagonal bracing, which he considers a defect; but the experience of the Menai bridge, in which the side railing is composed of a complete net-work of light iron, not less than 7 feet high, as represented in Fig. 6, sufficiently proves that no diagonal bracing of this sort can prevent those violent undulations, to which bridges not trussed on Mr. Clark's principle must always be liable.

That the Menai bridge, notwithstanding its great length between the points of support, has not yet been seriously injured, I ascribe to the peculiar system adopted by Mr. Telford, of making his suspension rods in several pieces with joints, which converts each of them, or at least all those composed of three or more pieces, into a sort of chains, thus giving them that sort of flexibility, which prevents their being alternately subject to such very violent compression and tension, as those of the bridge of Montrose, of which all the rods in the centre part, where the roadway was carried away by the fury of the hurricane, were entirely broken on one side, and those on the other side opposite to the fracture, generally bent, and probably also partially broken. The light wrought iron roadway bearers of the Menai bridge were also evidently less liable to be injured by those undulations, than inflexible bearers of cast-iron, such as those of the Montrose bridge would have been. But though the Menai bridge has hitherto escaped with no material injury, until the late tremendous hurricane of the 6th and 7th of this month, which was so destructive at Liverpool, and which has injured it, as I am told, very slightly, still it appears desirable that this magnificent national work should be secured for the future, by at least two

if not four lines of inflexible trussing, in all the central part of the roadway, between the two great towers of masonry which support the chains, and which for symmetry, though not from necessity, might be continued along the remaining parts of the roadway also.

I am informed that the particular portion of the roadway of the chain pier at Brighton, which has twice given way, has subsequently to its second failure been trussed by a vertical segment of wood over the central part of it, with chains led down from each end of this segment, and fixed to the two adjacent clumps of piles by which the towers that carry the chains are supported. But there being four inverted arches formed by the chains of the suspension pier at Brighton, unless the other three portions of the roadway be trussed also, I have no doubt but that some future hurricane will produce the same injurious effect upon one of these, though they have hitherto escaped.

As the injurious action of violent winds upon suspension bridges, if I am right in my views concerning it, must depend on the length and width of the bridge; and as hurricanes, though much less frequent, and generally less violent than in tropical climates, have occurred, and must again be expected in this country from time to time, I am of opinion that the roadway of every suspension bridge having only light side railings, such as those at Montrose, Aberdeen, &c., and indeed of all that I have seen, excepting that of Hammersmith, will sooner or later be carried away by some future hurricane, provided that it shall approach to, or exceed 400 feet in length between the points of support. The necessity of at least two inflexible lines of trussing for the roadway of such bridges has hitherto been so little attended to, that I thought it might be useful, at least to the junior members of this Institution, to bring it pointedly under their consideration.

Chatham, Jan. 29, 1839. C. W. PASLEY, Colonel R. Engineers.

NOTE.—Since the preceding paper was read I have been informed by Mr. H. R. Palmer, one of the Vice-Presidents of the Institution, who assisted Mr. Telford when the original plans for the Menai Bridge were under discussion, and went through the whole of the details with him, that the necessity of trussing the roadway was the subject of much anxious consideration; that Mr. Telford foresaw the probability of trussing being required, but finally decided upon omitting it in the first instance, and adopting it subsequently should experience prove it to be necessary.

C. W. P.

VIII.—*On the Supply of Water from Artesian Wells in the London Basin, with an Account of the Sinking of the Well at the Reservoir of the New River Company, in the Hampstead Road.*

By ROBERT W. MYLNE.

THE sinking of Artesian Wells is a subject in which the inhabitants of London have taken considerable interest during the last few years, and public Companies have been projected for the purpose of supplying a portion of the Metropolis with pure and soft water by that means.

The construction of Artesian Wells is now generally understood, many having been sunk in England, as well as on the Continent, where they obtained their name, from having been originally adopted in the French province of Artois, called by the Romans Artesium. These wells are made by boring vertically through a deep stratum of clay into one of sand, where water is generally found, and from whence it will rise to a considerable height, in many instances to the ground surface, and sometimes higher. This greatly depends upon the elevation from whence the sand stratum is supplied at the verge of the basin, within which the stratum of clay is situated.

The strata under, and around this Metropolis (designated as the London Basin) are peculiarly adapted for these wells, the whole being comprised in an immense bowl of chalk, many miles in extent, on the interior surface of which there is a thick lining of sand supporting a deep bed of clay, commonly known as the London Blue Clay; and upon this the Metropolis, and adjacent country, stands.

The sand which lies below this clay has always been found considerably charged with water, derived, no doubt, from the surface of the more distant country, and entering where the sand makes its appearance on the surface, which may be termed entering at the verge of the basin. This outcrop, or margin of sand, in some instances, is at a great height above the level of the Metropolis. It therefore necessarily follows, that, on boring into the basin, the water will rise in the bore-holes to various distances from the surface, according to the elevation of their respective situations.

Under these circumstances, most of the wells already sunk in and about London obtain their supply of water; and from this source it has been asserted that a very large quantity of water might be procured for the various uses of the inhabitants, while others consider it so very precarious, and the water so difficult to obtain, that they prefer boring down into the chalk, where they suppose an inexhaustible supply can be found.

With a view to obtain as much information as possible on this point, a well has been sunk by the New River Company: a brief account of which will be found in this paper, setting forth all the material circumstances which occurred during the execution of the work. But before entering on the detailed account of this particular well, it seems desirable to mention a few circumstances connected with other wells, which have come under the knowledge of the writer, and which, perhaps, may be considered to throw some light on the subject.

First, as regards those wells which are supplied from the deep *sand springs* below the bed of clay. There is one at St. Alban's House, Piccadilly, where the water rises to within about 50 feet from the surface of the ground; also another at the banking-house of Messrs. Coutts and Co., in the Strand, which formerly rose above the surface of the ground, and discharged itself into the Thames; but, from other wells having since been sunk in the neighbourhood, the water surface has lowered several feet, and at present is below the low-water level of the river. At Norland House, Kensington, there was one which ran over for many years; and when the spring was first struck, it was so strong that it washed the well-sinkers out at its mouth; this great overflow continued some time, but has now ceased from the effect of other wells sunk in the vicinity.

Instances have occurred around London of the water rising to great heights: at Tottenham there were several wells that ran over; and also at Tooting, Mitcham, and Hanwell; but all of these have more or less diminished in their discharge, and but few rise to within many feet of the surface of the ground which they formerly overflowed.

In the town of Cambridge, which stands upon clay, a considerable number of wells have been sunk, most of which rose above the surface; but they are now much reduced, both in height and quantity.

At Gloucester the depth of the wells vary more than in other places, although the town is situated on ground gently falling towards the river;

instances exist here where persons have obtained a good supply of water at a reasonable depth, while attempts to obtain a supply at a considerable depth, by their immediate neighbours, have utterly failed, after incurring great expenses.

Of those sunk or bored into the chalk, in the vicinity of London, the most remarkable as regards the strength of the springs, and the quality of the water, are those around Wandsworth. One in particular, at some Copper Works: where, from a large bore-hole, the water is forced out to the height of about 20 feet above the ground. For convenience it has been confined by a bent tube, so as to deliver the water horizontally into the mill-head. Several others occur along the banks of the River Wandle, all delivering above the ground surface, and so plentifully that they give considerable power to the mills on that river.

The proprietors of the larger wells, have generally found that there is an intimate connection between all that derive their supply of water from the sand stratum.

A striking instance of this is afforded by the well in Messrs. Calverts' brewery, in Thames Street, which is materially affected when Messrs. Barclay and Perkins work theirs in Southwark, although situated on the opposite side of the River Thames. Also the supply at Messrs. Whitbread's brewery, in Chiswell Street, which is one of the earliest wells of any magnitude, was so much inconvenienced from this cause, that they were compelled to bore to a very great depth, and thus obtain a more permanent supply through vertical pipes.

Messrs. Combe and Delafield's supply, in Castle Street, Long Acre, was also affected in this manner, and on deepening the well it completely destroyed another that had been sunk for the establishment of a saw-mill in St. Martin's Lane.

At one period, in sinking the well at the Hampstead Road reservoir (before alluded to), the water was likewise found to be sensibly affected to the extent of from 10 to 14 feet in height, by the working of an engine at the well at the Zoological Gardens, in the Regent's Park, which terminates in the sand.

It is needless to enumerate the many further instances that might be adduced to establish this fact; I shall, therefore, now advert to the difficulties that present themselves in obtaining water from the sand springs, when subjected to a considerable draught.

The sand is of so fine a nature that it will pass through the smallest apertures,

and is easily put in motion, from its specific gravity being little more than that of water, and, from its extreme sharpness, it is found to cut the pump barrels exceedingly; large quantities of sand will in this way be drawn up with the water from beneath the clay, thus forming large cavities below, and causing the clay to sink, by which the foundations of all buildings situated near the well are endangered.

A very remarkable instance of the subsidence of the ground occurred at the Hampstead Road well, where the quantity of sand raised by the engine through the 8-inch pump was such as to cause a very serious settlement in the large raised reservoir adjoining, by separating the high banks into two distinct portions; damaging a culvert, and snapping a line of iron pipes asunder. This no doubt would have affected the adjoining houses, had not the pumping been discontinued. A similar case happened at Messrs. Reid and Co.'s brewery, in Liquorpond Street, where the well, after the engine was set to work, during the time of sinking it, was found to have created such a cavity below, that the proprietors were obliged to close it almost entirely, to save their buildings from ruin. At the Vinegar works in the City Road, the well, from the same cause, was altogether abandoned for manufacturing purposes; as also a large well at the brewery of Messrs. Ramsbottom and Co., in Broad Street. At Whitechapel there was another well, belonging to Major Rhode, where it was found, on inspection, that the withdrawal of the sand, by pumping, had formed an immense cavity underneath the plastic clay; this caused a material subsidence of the ground, and 20 feet of the lower part of the brick shaft disengaged itself, and falling to the bottom, the fragments were completely buried in the quicksand.

Many other instances might be mentioned of wells having been abandoned, from the quantity of sand raised, and occasioning great loss of property, through the sinking of the surface-ground; but it will be needless to pursue the subject any further.

It therefore appears, that little dependence can be placed upon the quantity to be derived from the *sand spring*, and also that a great risk attends the obtaining it in large quantities. To obviate these difficulties, it has lately been considered advisable to sink through the sand into the *chalk*, which has been done generally by the means of boring, and the introduction of a small pipe; but as yet there is little experimental knowledge on the subject; and, from the few instances that have occurred in the Metropolis, a sufficient number

of facts have not been collected to enable a correct opinion to be formed, as to the quantity of water that can be obtained from such a source.

Among the larger wells that have been sunk into the chalk, there has been one lately executed at Brighton, for the supply of that town with water, which produces an abundance of fresh water from below the level of the sea. The water here issues from the fissures in the chalk, which are intersected by horizontal headings driven from the bottom of the well, from whence the water is pumped up for the supply of the town.

A singular circumstance happened in cutting through the chalk hills, for the formation of a tunnel for the Thames and Medway canal: that operation had the effect of draining the whole of the fresh water from the wells within the range of a mile, and substituting salt water in those wells.

A large bore was also made in the chalk valley through which the River Lea passes in Hertfordshire, where the layers of flints in the upper part of the chalk yielded a small quantity of water; but no increase took place beyond the depth of 100 feet, although the well was continued to the depth of 300 feet, at which point the chalk marl began to make its appearance; but the tool having broken in the bore, the work was abandoned.

In Paris, where a basin of clay exists similar to that of London, a bore-hole has been in operation for a long period, and is at present being carried on at the depth of 1360 feet in the chalk, and 1490 feet below the surface of the ground; as yet no quantity of water has been obtained.

It will therefore appear, from the few instances which have been mentioned, that different situations will be affected by varying causes; and from the little information that can be collected, with regard to sinking wells in the chalk, no rule can at present be laid down upon which an opinion can be formed as to the possible results of an experiment at any particular place.

Having offered these preliminary observations on the subject of wells, I take leave to subjoin a section of the well lately sunk at the New River Company's reservoir, in the Hampstead Road; together with some sketches of the tools used during the works, and a statement of the mode in which the work proceeded. The execution of this work was placed under the immediate care and superintendence of Messrs. Hunter and English, experienced millwrights, whose attention was unremitting during the operation.

PARTICULARS OF THE SINKING THE WELL AT THE HAMPSTEAD ROAD RESERVOIR, THE PROPERTY OF THE NEW RIVER COMPANY.

WM. C. MYLNE, ENGINEER.

On the 21st March 1835, the well-sinkers commenced excavating in made ground, to the depth of 6 feet, and continued through 17 feet of red (Hampstead) gravel, at a diameter of 20 feet; the sides of the shaft were supported by strong wooden curbs of a less diameter than the excavation, and clay puddle was filled in behind them in order to shut out the land springs. These curbs were sunk down through the bed of gravel, and made to enter a few feet in the London blue clay lying immediately below it. At this depth a cast-iron footing was fixed within the wooden curbs, upon which a 14-inch brick shaft was carried up to the ground surface, of a clear diameter of 12 feet 6 inches, worked in Roman cement, and the cavity of 6 inches, that remained between the back of the brickwork and wooden curbs, was carefully filled up with concrete.

The excavation was continued through the stratum of blue clay at a reduced diameter of 12 feet 6 inches, and steined with 9-inch brickwork in cement; thus leaving a clear shaft of 11 feet diameter.

The brickwork was built by continual underpinnings, as the work proceeded; and cast-iron rings were inserted at about every 8 feet in depth, projecting beyond the back of the brickwork a few inches into the clay, for the purpose of supporting the shaft as it progressed.

On attaining the depth of 57 feet in the clay, the boring auger, that was always kept about 6 feet in advance in the centre of the well, gave indications of having passed through the blue clay into a stratum of soft mottled clay; it was therefore thought advisable to discontinue the brickwork, in order to ensure a foundation for its support.

On proceeding with the excavation, the diameter was again reduced to 10 feet 9 inches, for the purpose of introducing, as a substitute for brickwork, cast-iron cylinders, formed of 6 segments, 6 feet in length, united by bolts through flanches projecting on the inside, leaving 9 feet 9 inches clear diameter; these were joined together, and being forced down by hand-screws, were made to follow the work as the sinking continued in the remainder of the blue clay,

and then through a 10-feet bed of soft mottled clay: on approaching the bottom of this stratum, water first made its appearance.

An engine and two 8-inch pumps, in two lifts, were erected and set to work, by which the well was kept dry. The sinking was thus continued with the same sized cylinders, following the work to the further depth of 8 feet, in a bed of fine brown sand: during the progress through this sand the quantity of water considerably increased, occasionally accompanied by large quantities of sand blowing up above the lower edge of the cylinder; this was observed chiefly to come from the north-east; and on boring a hole in the cylinder at that quarter, in the mottled clay stratum, it was found that the clay had fallen down, leaving a cavity extending 3 feet beyond the cylinders. From the very unequal pressure, occasioned by this cavity, the cylinders were forced out of the perpendicular, and became completely jambed when their lower edge was within a foot of the bottom of the sand stratum.

From this circumstance it was found necessary to commence with cylinders of a sufficiently reduced diameter, so as to be placed within the others, to allow them to sink perpendicularly.

During the period that elapsed in preparing these smaller cylinders the water rose to within 85 feet of the surface of the ground, and the fine brown sand within the cylinder to nearly the level of its original surface.

On this second set of cylinders being ready, the water was pumped out of the well, and the sinking proceeded with through the accumulated sand and the remainder of the sand stratum, and entered another bed of mottled clay, similar to the former. There was then found a gut of sand on the north-east side, upon the surface of the clay, to the depth of 2 feet; extending 4 feet on the circumference of the well, and diminishing towards the centre.

During the sinking of the cylinders through this bed of mottled clay, the water and sand entered in large quantities from the same side as the gut, to the great inconvenience of the workmen; and on reaching the bottom of the stratum, which was 19 feet 6 inches in depth, they came upon a thin layer of pebbles, so closely embedded in black sand as to form a complete mass of stone, which was broken through with difficulty by hammers and chisels: the layer being from 9 to 12 inches in thickness, and the pebbles averaging about the size of an egg: on leaving this, a bed of dark brown sand presented itself, and was sunk into, to the depth of 4 feet 6 inches, from which there was little

increase of water beyond that still continuing to run from the north-east side. This run of water was found, on inspection, to have occasioned a similar cavity behind the cylinders in the last bed of clay, as that before described in the upper bed, with the same attending consequences, the cylinders being forced out of the perpendicular, and again jambed in the sand; when, after several ineffectual attempts to force them down by powerful hand-screws, it was found necessary to commence with a third set of cylinders, of a further reduced diameter of 7 feet 4 inches.

The sinking then proceeded through the remaining 7 feet of dark brown sand, and into a bed of quicksand, of a darker colour, to the depth of 5 feet; where, on the 19th January 1836, the cylinders became so jambed that it was again found impossible to sink them lower.

During the latter portion of this work great difficulty was experienced from the frequent blowing-up of the sand, often to the height of 6 feet at a time: and also from the immense quantity of sand that was pumped up with the water, continually choking the pumps. This had caused not only the large cavities already mentioned behind the cylinders, but the lower part of the brick shaft was observed to have inclined bodily towards the north-east; thus causing numerous serious cracks, and in some places so affecting the shaft that it became of an oval figure. Several segments of the cast-iron cylinders were also broken asunder at their vertical flanches. This led to an examination behind the brickwork. An opening was made in the brick shaft, about 60 feet from the surface, where a cavity was found, extending many feet at the back of it; this was immediately filled up with stones and brickbats, concreted with lime and gravel. Several lengths of cast-iron cylinders were then placed on the top of the second set, for the purpose of lining the brick shaft; and all the irregular spaces between them and the brickwork were filled up with good concrete.

This settlement of the ground not only extended itself to the works in immediate connection with the well, but also to the reservoir banks contiguous, which were found to have sunk so much, that a large portion of the embankment and inside lining of brickwork, on the side nearest the well, was separated from the remainder, and the cast-iron main beneath the bank was broken in several places.

From the circumstance of these extensive settlements, it was considered advisable to discontinue the pumping, and thus suspend the works for a short

time, in order to consider of some other mode of getting through the remainder of the sand stratum; of the many plans suggested one was adopted, by the recommendation of Mr. Simpson (engineer to the Chelsea water-works), whose Report is annexed. It is there proposed, that the sinking should be continued with the water remaining in the well.

In accordance with this recommendation, on the 22nd August 1836, the 8-inch pumps were taken out, and a wrought-iron cylinder, or tube, formed of boiler-plates, rivetted together to the length of 62 feet, with a clear diameter of 5 feet 10 inches, was lowered to the surface of the sand, which had risen 7 feet in the well since the discontinuance of the works; the level of the water, which varied considerably at times, generally stood about 102 feet from the ground, thus leaving a column of water of 29 feet in the well.

Across the top of the tube, which stood 33 feet above the water, a platform was placed for the workmen to carry on their operations.

The sand now having the pressure of water upon it, was found to have become hard, requiring to be loosened by chisels. After that, the sand was excavated by an instrument called a miser: the different misers employed varied in size and figure, but were all constructed on the same principle, and each capable of holding about 2 bushels of sand, although they seldom came up more than half full.

In this manner the sinking was continued, while the wrought-iron cylinder was forced down by hand-screws that were placed on its upper edge. After thus forcing through the remainder of the quicksand stratum, a bed of sand, intermixed with small flints and pebbles, was sunk into, to the depth of 7 feet; on passing through which a bed of chalk was entered, containing numerous small flints, very closely embedded, to the depth of 4 feet 6 inches. This stratum was excessively hard and difficult to penetrate, thus rendering the progress of the works exceedingly slow: at the bottom of this stratum the chalk became much more solid, and the cylinder was sunk into it, to the depth of 12 inches; at which period the works were discontinued for a short time, leaving the well at a depth of 151 feet from the surface of the ground.

During the sinking through these different strata the wrought-iron cylinder had been lowered on an average from 4 to 5 inches per day, and the works kept constantly in operation.

It was now considered that the water and sand would be entirely shut out, and that the sinking might be continued in the usual manner. The platform

was then cleared away, and two 8-inch pumps, in two lifts, were put up within the wrought-iron cylinder, and on its being set to work by the engine the well was freed from water.

Towards the end of March 1837, the workmen commenced excavating in the hard chalk, at a diameter of 6 feet. The chalk was now found so solid as to require no lining; and, as the works proceeded, layers of flints were cut through, running about 2 feet 6 inches apart, and from 9 to 12 inches in thickness: each of these layers yielded water from their under-side, and they continued at such intervals to the depth of 33 feet, or 183 feet from the surface of the ground.

At this depth, one of the layers being partly broken into, produced more water than heretofore; and from this extra quantity (added to that yielded from those above) the 8-inch pump was found incapable of keeping the water lower than within 7 feet of the bottom.

The sinking was therefore discontinued, and attention was directed to the better securing and finishing the works above. The chalk was excavated to an enlarged diameter below the bottom of the wrought-iron cylinder, for the purpose of forming a brick footing, of a diameter of 4 feet 7 inches in the clear, increasing at the bottom to 7 feet 9 inches, being 10 feet 6 inches in length; on the top of this brickwork a broad cast-iron ring, in segments, was fixed, projecting a few inches beyond the back of the brickwork into the chalk, upon which rested the cast-iron cylinders that were introduced within the wrought-iron tube for the purpose of strengthening it, and to guard against the admission of sand, in case of its failure from corrosion.

These cylinders are of a clear diameter of 4 feet 7 inches, and were erected one over another to the height of 35 feet; and the 31 feet of wrought-iron tube that remained standing above them was cut off, leaving 31 feet of it in the well.

It was now considered advisable, in order to obtain as much water as possible, to admit such quantity from the sand-spring as was practicable without the admission of sand; to effect this, three $1\frac{1}{2}$-inch cocks were inserted in the cylinders nearly at the top of the dark brown sand stratum, and the water flowed through them mixed with a small quantity of sand.

The whole of the works having now been properly secured and made complete, about the end of February 1838 the 8-inch pumps were taken out and two of 12 inches diameter introduced, in two lifts. During a considerable

time that elapsed in preparing for the erection of these pumps, the water surface in the well had remained at a level of 108 feet from the surface of the ground. In the month of August 1838, being the period at which the springs are short, and having also been preceded by a dry winter and summer, the engine was set to work, and the result of two weeks' experiment, as to the quantity of water raised, is shown below. Another experiment was also made in March 1839, being the period at which the springs usually produce their utmost, and following a rainy winter season, the result of which is also annexed.

Engine set to work 1st August 1838.—Surface of water in the well 109 feet from the ground.—Depth of water in the well 74 feet.—One 12-inch column of water, in 2 lifts.

	Cubic Feet.
Quantity of water raised during the first week . .	103,950
Ditto ditto second week .	104,625
	2)208,575
Average cubic feet raised per week	104,287

Or 14,898 cubic feet per day of 24 hours.

Engine set to work 20th March 1839.—Surface of water in the well 105 feet from the ground.—Depth of water in the well 78 feet.—One 12-inch column of water, in 2 lifts.

	Cubic Feet.
Quantity of water raised during the first day . . .	30,584
Ditto ditto second day . .	30,415
	2)60,999
Average cubic feet raised per day of 24 hours . .	30,499

SUMMARY of the Expenses incurred in sinking the well, erecting an engine and pumps, and making good all damages occasioned during the execution of the work:—

	£.	s.	d.
Excavations, cartage, labour, &c.	3,006	2	0
Steam-engine and machinery	1,912	17	10
Millwrights'-work, well-sinkers, castings, &c.	5,762	8	6
Bricklayers' and masons' work	1,740	5	9
	£12,421	14	1

The following table shows the depth of the different strata of earth, through which the well was sunk:—

	Feet.	In.
Made ground	6	0
Red gravel	17	0
London blue clay	59	0
Soft mottled clay	10	0
Fine brown sand	9	0
Soft mottled clay	19	6
Black sand and pebbles	1	0
Dark brown sand	11	6
Dark quicksand	6	0
Sand, with flints and pebbles	7	0
Chalk, with small flints	4	0
	150	0
Chalk, with flints, as far as has been excavated .	33	0
Total depth	183	0

Trinity high-water mark, 77 feet 8 inches below the surface of the ground.

REPORT OF J. SIMPSON, Esq., ENGINEER.

Sir, Thames Bank, Pimlico, March 30th, 1836.

Having, in pursuance of your request, surveyed the well and works at the New River Company's Hampstead Road reservoir, and obtained the necessary information, I have now the pleasure to submit the following Report.

With respect to the stability of the well and works, I have to observe, there is a settlement in the second brick curb, about 29 feet from the surface; towards the bottom of this curb it is very much distorted, and there is every indication of its having been forced over towards the reservoir by the pressure of the earth.

The well-diggers have had to contend with much more than the ordinary obstacles generally encountered in such undertakings; the subsidence of the soil immediately contiguous to the curb and cylinders has been great, and notwithstanding the precautions adopted of sustaining them and the pumps by long iron bolts, serious injury has accrued to the curb and cylinders, and the well should not, in my opinion, be proceeded with until they are effectually secured.

It has been stated to me, that, during the sinking of the lower cylinder, the sand was continually forced under it into the well, whenever the spring got vent, more especially on the side next the reservoir; and there are sufficient indications on the surface to show that the subsidence of the earth has been very extensive—there is no doubt but that the settlements in the reservoir have been caused by it; and from the appearance of the walls of the cottage, the subsidence has also proceeded in that direction; and, although difficult to ascertain its precise limits, it seems to me we may conclude that it ranges from one to two hundred feet round the well; the quantity of sand dug out from the bottom appears greatly to have exceeded the cube of the well at the depth of the lower sand stratum: from the state of the water I saw pumped out on the 17th inst., it contained from $\frac{1}{8}$ to $\frac{1}{12}$ sand and clay, the colour of the latter being frequently discernible; and from the occasional gushes of clay, sand, and water, through holes which had been bored in the lower cylinder, to prevent its flowing over the top, on to the well-sinkers, it is manifest there is a great subsidence of the soil round the curb now going on, and that it proceeds most rapidly when the water is pumped out of the well.

From examples of wells bored and sunk in and near the Metropolis, particularly on the western side, viz., Hammersmith, Fulham, &c. &c., the springs in the chalk formation are evidently more abundant, and in most instances rise much higher than the springs from the sand stratum above it; nearly all the overflowing wells near London derive their supply from the chalk formation; the water in most cases is found at about 70 feet depth in it, sometimes in strata of loose flints, sometimes from fissures, and occasionally from soft veins in the chalk. Sufficient proof of this has been obtained by borings in and near the Metropolis, and by connecting the observations with the cases of wells actually sunk into the chalk immediately outside the London basin; the fact of the water rising to higher levels may be attributable to the more open structure of the formation compared with the sand strata, but it is more probably attributable to the sources from whence the supplies are derived being in more elevated situations; and there seems already sufficient evidence to connect a considerable portion, if not the whole, of the sources of supply with the rivers and streams traversing the bassetts of the strata, where they crop out and intermingle with the chalk on the uplands near the Metropolis.

The chalk formation is evidently the stratum from which large supplies of water can reasonably be anticipated to effect the object; however, so far as my practice has directed me, a shaft must be sunk, and adits driven, to open and unite the fissures in what may be really termed the cavernous structure of the chalk, to admit of the water flowing to the well as freely as possible. The difficulty and expense of sinking a shaft through the sand strata in or near London has hitherto been a serious obstacle: boring has been the expedient resorted to; and although, by this process, large supplies of water have been obtained, I am of opinion the real capabilities of the chalk formation, as a water-bearing stratum in this part of the country, have not yet been fully developed.

To return to the object more immediately in view, viz., the sinking of the well in the Hampstead Road safely into the chalk, I have to state, that the work has occupied my most serious attention, previously to the adoption of any plan. I am of opinion, it will be *advisable to secure the present brick curbs and upper cylinders* with strong timber segments and strutts; and wherever derangements of the curbs and cylinders are discernable, to fit in and substantially fix strong scantling vertically between them and the segments, to counteract the lateral thrusts now acting against the curbs and cylinders

from the pressure of earth on the opposite sides to the great vacuities in the soil contiguous to the well.

The surrounding soil will, no doubt, be saturated with water to the utmost extent until July; and I recommend further proceedings to be delayed until the commencement of that month, when it may be reasonably anticipated that the soil will become more consolidated, and be in a state more suitable for the prosecution of the undertaking.

With regard to the recommencement of the work, only two of the plans which have occurred to me are deserving attention.

1st. The driving of an iron pile curb.

2nd. The sinking of iron cylinders cast in entire circles.

The model of the iron pile curb you shewed me appeared very well designed for the purpose in view; the driving it, however, would be a work most difficult to execute.

According to the section of the strata before mentioned, which, in the absence of better information, I must quote, the bottom of the third cylinder may be taken to be from 13 to 14 feet above the chalk; and, as it is not practicable to keep the sand lower in the cylinder than 6 feet above the bottom of it, the drift of the curb, with 3 feet in the chalk, will be from 22 to 23 feet. I do not see how such an extent of drift can be effected with less than three tier of pile curbs, especially as the water must be kept pumped out of the well for the performance of the work, which will throw considerable pressure against the piles; and the quantity of sand which will inevitably be forced under the cylinders with the water, will prove a serious interruption, independent of causing the further subsidence of soil round the well, added to which the concussions arising from the blows of the ram of a pile-engine at such a depth will affect the mass to a serious, if not to a dangerous extent. I cannot, therefore, recommend this mode of proceeding.

The sinking of iron cylinders through sand and gravel, without pumping out any of the water within the shaft, is now a very common practice among the well-diggers; it is performed with the common boring rods and tools, the shells, or buckets, are fitted with valves at the bottom to open upwards; they are much larger than those used in borings, and the men turn and force them into the strata and draw the material up in them with the greatest ease; when the cylinders become set, they make use of a small sling ram, or occasionally of a very heavy sledge-hammer, to jar them, and they resort to this expedient

whenever they find the cylinders do not sink according to the proportions of sand or gravel they remove. The keeping of the water in equilibrium inside and outside the cylinders is very important; I have practised this method, and from my observation of its efficacy in several instances, I recommend its adoption at the well in the Hampstead Road; it may involve the necessity of two sets of cylinders, but I think, if the first be made strong enough a second will not be required. The top of the cylinder is always kept above water, which will be an advantage in the case in question; it will enable the men to work in a part of the well where there is much more room, and afford them greater facilities for their operations.

In case my observations have not extended to any point, or to every particular, you directed my attention to, I request the favour of you to inform me, that I may supply the deficiency.

I remain, Sir,

Your most obedient servant,

(Signed) JAMES SIMPSON.

Wm. C. Mylne, Esq., Civil Engineer,
New River Head.

IX.—*Account of the Gravesend Pier.*

By WILLIAM TIERNEY CLARK, F.R.S., M. Inst. C. E.

Previous to the introduction of steam packets on the River Thames, Gravesend was a place of resort for many persons from London in the summer months; the passage by the sailing packets, or tilt boats, as they were anciently called, presented an agreeable but uncertain conveyance. This transit was never attempted except with the tide, and frequently the passengers, being becalmed, were compelled to land at some place short of the vessel's destination, or to remain on board until the following tide; and it was not uncommon to be detained on the water throughout the night, especially when the journey commenced after mid-day. These sailing packets were of a burthen varying from 15 to 35 tons, and were capable of carrying from 60 to 100 passengers. A coach, called a tide coach, awaited the arrival and departure of the packets, for the convenience of those who resided at Rochester, or other places in the immediate vicinity of Gravesend. At the period referred to (1820) the population of Gravesend and the adjoining place, called Milton, was under 5000; and the visitors were chiefly of the lower classes of society. These packets either drew too much water, or it was considered inexpedient to allow them to lie alongside the jetty; and, consequently, in order to embark or disembark, the passenger had to hire a small boat or wherry to convey him between the shore and the packet, for which each person paid sixpence, although the distance would not exceed from ten to twenty yards, and although the same wherry would convey eight persons. The passage between Gravesend and London generally occupied from five to six hours, because, though a fair and fresh wind might have carried the vessel the whole distance in three hours, these favourable circumstances were invariably made the cause of delay in starting, in order to get a larger number of passengers, or, in the words of those days, "a good tide;" the whole object of the Captain (who was also generally part owner) being to reach the terminus by the time the tide was exhausted. When a steam vessel first came to Gravesend the attempt was made to lay her alongside the jetty, so that the passengers

might walk in and out; but this attack upon vested rights was so stoutly resisted by certain parties, that the owners of the packet soon made an unconditional surrender; and the watermen, seeing that their easy earnings had been placed in jeopardy, reduced their fare between the shore and packet to fourpence each person. The trip to and from Gravesend, however, being rendered certain by the steam packet, the resort to the place gradually increased, and at length, in the year 1830, in consequence of continued remonstrances by the visitors against being compelled to use the wherry boats, and a general demand for a pier, a public meeting of the inhabitants was called by the Mayor, to consider the propriety of erecting one. In the notice for convening the meeting, however, the Mayor thought it expedient to disclaim giving any judgment of his own, and to throw out a suggestion that, provided the pier should be built, compensation should be made to the watermen. The meeting was fully attended; but most of the persons who signed the requisition either were absent, or became at the moment opposed to the erection of a pier, and the measure was consequently condemned; for not more than four or five inhabitants were bold enough to declare their real sentiments, among whom was the then Town-clerk, who, for his temerity, was dragged from the Town Hall, and assaulted by the mob.

Notwithstanding this discouragement, a meeting of proprietors of the steam-packet companies was convened early in 1831, to express their opinion upon the utility of a pier; and although individual interest very much encouraged the hope of success, intimidation and prejudice prevailed to such an extent, that the steam-packet proprietors declared that a pier would be injurious to their establishment.

Public opinion, however, pressing closely, the municipal authorities of Gravesend, in 1832, applied to the legislature for authority to erect a pier. Perhaps a greater parliamentary contest on a subject of local improvement never occurred; nor were stronger feelings and interests ever called forth. The promoters of the Bill had opposed to them many of the inhabitants, as well as the watermen; who, with the mariners generally, declared the proposed plan to be impracticable, on account of the obstruction to the navigation, and as tending to impede the supplies for the London coal and fish-markets. The leading argument of the opponents of the plan was, that, as it was proposed to carry out the new pier to a considerable extent beyond the existing quay and jetty, the flow of the tide would be obstructed thereby in a much greater

degree than by the old jetty with its numerous arches. This was, however, met on the other side, by proof that there was a much greater extent of opposing surface in the old jetty than in the proposed pier, although the latter was to extend 40 feet beyond the lowest point of the jetty. Some of the authorities, also, connected with the conservancy of the Thames, took active measures of opposition; and, finally, at the close of the session, after powerful struggles and divisions in every stage, and an expenditure of a large sum of money, the Bill was lost in the House of Lords by a majority of one.

The struggle in Parliament, however, had called up the notice of the public, and even of the Ministry: and in consequence of some particular features in the report of the Lords' Committee, the Duke of Richmond, then one of the Cabinet, prevailed on their Lordships to recommit the Bill. But, after a second expensive struggle in the Committee, it met with a similar fate, and was consequently lost.

The contest, however, was not without its advantages, for, in the course of the investigation, the authorities of Gravesend became not only confirmed in the propriety of their conduct, but received so much public encouragement, that they determined, during the recess, to erect a temporary pier, and to shew to Parliament, in the ensuing session, not only the practicability of the proposed pier without injury to the navigation, but also the local as well as public benefit which would follow such a measure.

Accordingly a wooden pier was speedily constructed, and immediately produced such manifest advantages to the inhabitants, by more than doubling the resort to the town, that converts to such an improved landing were daily made, and those steam packets which did not use the pier had but few passengers. The watermen and their families (calculated at 1500 persons) were in the greatest excitement; and though they menaced several of the parties engaged in the pier, they refrained from acts of open violence, in the full conviction that Parliament or the Admiralty, in the following session, would either suppress the pier, or that, during the winter, one of those misfortunes (predicted by them) of ships striking against it and lives being lost, would effectually and for ever remove it. The expectations of many persons were greatly raised by the fact of the *United Kingdom*, of 400 tons burthen, being totally wrecked in the month of January 1833, at Northfleet, about two miles to the west of the pier, and being for three days carried by the tide up and down the river between Northfleet and the east end of Gravesend; but, contrary to all expec-

tation, the pier sustained no injury; and thus was the fact fully established that, so far as the ebb and flood tides were concerned, the pier projection was no impediment to the navigation.

In 1833, a Bill was again brought into Parliament, and again opposed; but at length, upon the Committee in the Commons passing the Bill, the watermen, aided by several of the inhabitants, proceeded in the night to destroy the pier, which they effectually did, after extinguishing the lights in the town, by cutting away with saws and hatchets, the piles that supported the platform. Such was the violence and menace used, that the civil authorities were utterly unable to put a stop to the destruction of property, or to preserve the peace of the town; and the presence of the military from Tilbury Fort, the Rifle Brigade from Chatham, and the Cobham Yeomanry were found necessary for several weeks, whilst the pier was repaired and order restored.

By this act of violence the opponents of the pier lost much of their Parliamentary support; and the Duke of Buccleuch, the Lords Salisbury, Strangford, Faversham, and Wynford, with many others who had opposed the Bill, now gave it their support, and ultimately the Act passed, in June 1833; and on the 29th of July of the following year, within 13 months afterwards, the present pier was opened to the public.

The advantages to Gravesend consequent on this event have been immense, and very far exceed the calculations upon which the claim for a pier were founded. The resident population is now more than 17,000, and in the summer season it exceeds 20,000. The buildings have increased in the like proportion, and the coaches between Rochester, Maidstone, &c. and Gravesend, attending the arrival and departure of the steam packets, average forty daily throughout the year.

The residents and visitors each month are found to be gradually increasing, whilst the latter are no longer confined to the mechanic and artisan, but they consist of all classes of society, and thus become a source of wealth and prosperity to Gravesend and the adjacent district of Milton, and afford great gratification to all who had to endure the labour and trouble of obtaining for the town its present pier; whilst the public, instead of paying sixpence each for the compulsory use of a small, inconvenient, not to say dangerous landing wherry, pay but threepence toll for a safe and commodious pier, which payment of toll is included in the fare of the steam-packets.

The following statement of the numbers of passengers landing and em-

barking at Gravesend for the last ten years, will prove the rapid and perhaps unparalleled increase, caused by the combined advantages offered by steam navigation and a commodious pier:—

1829	122,880	Before a pier was erected.
1830	225,600	
1831	237,600	
1832 / 1833	479,280	
1834	313,896	
1835	809,169	Since the pier has been erected.
1836	834,803	
1837	784,763	
1838	872,721*	
1839	1,005,430*	

GENERAL DESCRIPTION OF THE PIER.

The nature of the communication betwixt the quay and the water, previous to the erection of the pier which forms the subject of the present communication, will be fully understood from the plans and sections (Plate I.) Stairs descended from the quay wall, and from the bottom of the stairs a jetty extended to the low-water line of spring tides, or to a distance of about 116 feet from the front of the quay wall. The pier extends to a distance of 161 feet from the front of the quay wall, at which distance there is a depth of about 6 feet at low-water spring tides, and of 27 feet at high-water spring tides.

The centre line of the pier is represented by the line A B b, and the nature of the site and the extent of the erection will be seen at once from the general plan and longitudinal section of the foundations (Plate I.) The ground upon which the pier was to be founded was very limited, and the existing quay was to form part of the new pier. Also a separate jetty and stairs were to be provided for the use of the watermen. The general arrangements by which these objects were effected, are shewn by the elevation and general plan of the quay and pier (Plate II.) The pier consists of two parts, the promenade and the T head. The promenade, or part of the pier between the T head and quay is

* These numbers for 1838 and 1839 may not be strictly correct, but they are derived from the best sources of information that could be obtained.

127 feet in length, and 39 feet 6 inches in breadth, and supported by four arches resting on columns, as will be described hereafter. The T head, so called from its shape, on the extreme or northern end of the pier, is 29 feet 7 inches in width, and 73 feet in frontage to the river. It is supported on eighteen cast-iron columns. The descent to the stairs for embarking and disembarking from the vessels, is in the centre of the extreme end of the pier (Plate III.)

Watermen's Causeway. The causeway at which the watermen land at all states of the tide is 111 feet 6 inches in length, and 12 feet 5 inches in breadth, supported on seven cast-iron frames resting on separate foundations, and on transverse bearing beams attached to the cast-iron columns which support the promenade, four of which are provided with bracketed flanches for this purpose. The ground being excavated to a sufficient depth to secure a good foundation, an entire course of 3-inch York paving-stone, well bedded in mortar, and set quite level, was laid, holes having been previously cut through the stones for receiving the hold-down bolts; upon this the brickwork was laid, capped with Bramley Fall stone, 18 inches wide and 12 inches thick, with holes for the hold-down bolts to secure the frames which support the causeway. The end of the causeway near the quay wall, and the steps up to the quay, are supported on three similar foundations of brickwork parallel to each other, the intermediate spaces being filled up with concrete (Plate I.)

The cast-iron frames are firmly secured down to the brick and stone-work by four iron bolts, passing through the foundations, and the transverse beams are fitted endways between the columns, and screwed down to the flanches by 1-inch screw-bolts and nuts; longitudinal fenders of oak timber, 9 inches square, are fixed at the outside corners of the frames, and to the transverse pieces between the columns by 1-inch screw-bolts and nuts; between these longitudinal fenders are three cast-iron beams, each of which rests with one end on one half of the transverse beams between the columns, and is supported at the other end and at intermediate points on the frames, and securely fixed by dovetail keys. The whole is covered with 4-inch York paving.

Foundations of columns of Promenade. The promenade rests on cast-iron ribs forming four arches, supported by iron columns on brick and stone foundations (Plates I. & II.) The ground being excavated to a good and sound bottom, the entire surface of the excavation is laid with Bramley Fall rag-stone, dressed 9 inches in thickness, and no stone less than 4 feet square, set level in every direction, and well bedded in mortar. The stones are pierced with holes 4 inches square, for

receiving the hold-down bolts, each of which is provided with a cast-iron plate 15 inches square, well bedded to the under side of the stone. The brickwork being carried up to the proper height, is capped with two Bramley Fall stones, each 5 feet 9 inches square and 2 feet thick. Holes are also bored through these, the vertical spaces left in the brickwork being made to correspond with the holes in the lower stones, and great care was taken in setting out the holes and keeping the bolts in a perpendicular direction, and so wide as to correspond accurately with the centre of the bolt-holes in the bottom of the columns.

The sum to be expended by the Corporation on the new pier, was not sufficient to admit of the use of caissons or a coffer-dam, and the obstruction which they would have caused to the navigation was an additional objection to their use. Recourse was consequently had to a more simple mode, which has proved from experience equally advantageous and far less expensive, and which, it is believed, was never before practised. The complete success of the method, and the great advantages which may attend its adoption in particular cases, will be seen from the following account.

Foundations of T head. The columns of the T head are supported on fifty-four cast-iron piles, each column being supported on three piles. The stratum into which the piles are driven, consists chiefly of chalk, and as it was of the greatest importance that this part of the work should be executed with accuracy, in order that the centres of the piles, when driven, should correspond with the centre of the holes in the flanches at the bottom of the columns, the following precautions were adopted. A large platform of whole timbers was formed in length, breadth, and dimensions, as shewn in the plan (Plate I.), and well secured by 1-inch screw-bolts and nuts. Upon this, was accurately set out the distances between the centre of each column forming the T head of the pier. Plates of cast-iron 1½ inch thick, eighteen in number, and each having three circular holes, 17½ inches diameter, called guide-plates, were then screwed down in their proper situations upon the platform; the centres of the holes in each plate being adjusted so as to correspond exactly with the centre of the site for each pile.

The platform was then floated out and secured by moorings in its proper place, at the level of low-water spring tides, until four piles were driven, one at each corner, for securing it more permanently.

This being done, a cast-iron shell was introduced perpendicularly into one of the three holes of the guide-plates, and driven through the mud into the chalk below. An auger was then inserted, varying from nine to ten inches in

diameter, according to the density of the chalk, and the boring was proceeded with to within one foot of the length of the pile intended to be used. The depth of the boring varied according to the length of the piles, and other circumstances; the pile was then introduced through the shell into the bored hole, and driven down to within two or three feet of the top of the shell, when the shell was drawn up, and the pile driven down by means of a hard wooden dolly, made to fit the top of the piles, and upon which the monkey of the pile-engine acted. The greater portion of the piles for the T head were driven in this manner under water, in consequence of the prevailing wind during the months of February, March, and April, being easterly, which prevented the tide ebbing out to its usual extent, so that the platform and piles were seldom seen except when the wind shifted to the west, and oftentimes when it did it was for so short a period, that little could be done before the return of the flood. Under these circumstances, as it was evident that if favourable winds were waited for much time would be lost, it was determined to remove the platform. This having been removed, a wooden cylinder, 9 feet diameter, and 9 feet long, was made of 3-inch deal battens, firmly keyed and hooped together, the lower end being shaped like a sheet pile, and shod with iron. This cylinder was lowered over one set of three piles, and loaded sufficiently to cause it to sink through the soft mud of the shore, when it was driven into the hard ground. The water was then pumped out, and the mud removed low enough to enable the workmen to reduce the heads of the piles to a uniform level by chipping, so that the bases of the columns were fitted down to the tops of the piles metal and metal, and this operation was repeated as often as required. But the piles were found to have been driven with such accuracy, that not more than six required chipping, and the greatest variation did not exceed $\frac{3}{4}$ of an inch.

Adjusting Plate and Columns. When the pile heads of each set were adjusted, a cast-iron plate, connecting the three piles together, called the adjusting plate, was firmly keyed on immediately below the top of the piles (Plate IV.) The columns were then fitted down to the adjusting plate by $2\frac{1}{2}$-inch screw-bolts through flanches at the bottom. The holes in these flanches were a little elongated, so as to allow of the columns being slightly shifted and fixed in their true position (Plate IV.) Upon the columns rest the deep bearing-beams which support the superstructure. These bearing-beams are screwed together at the corners by $1\frac{1}{2}$-inch screw bolts, and fastened by bracket flanches to the capitals of the

columns; on their upper sides are flanches on which the flooring of the T head is laid. The columns of the T head are tied together at about one-third of their height, by timbers fastened to bracketted flanches on the columns. On these timbers are supported the frames for the stairs in the T head. The details of the adjusting Plate, the columns, the deep bearing-pieces, and the stairs, are fully shewn in the accompanying Plates.

Arches of Promenade. Upon each of the columns of the promenade rests a cast-iron rib 40 feet in length; each arch is composed of two of these ribs, secured together at the centre by $1\frac{1}{2}$-inch screw bolts and nuts. The whole structure consists of four such arches, kept apart at their proper distances by means of distance pieces (Plate VI.), nicely fitted to the ribs, and secured by $1\frac{1}{2}$-inch screw bolts. The ends of the ribs next the quay are secured by means of a square flanched piece built into the wall, and by hold-down bolts; four horizontal and vertical holes being made through the springing of the land arch, for this purpose. The ribs at the other end of the promenade, form part of the T head, as well as one-half of the arch, and are secured to the columns and deep bearing-beams. The different portions of the iron-work of the arches, and superstructure of the T head, were fitted together in a temporary manner at the iron-foundry, and when it came to be fixed upon the columns, scarcely any chipping was found necessary; indeed, many of the holes in the capitals of the columns were drilled from the dimensions of the working drawings, and found to correspond with the holes in the flanches of the deep open beams.

Floorings, Stairs, and Superstructure. The top edge of the arches of the deep bearing-beams of the T head being perfectly level, the wooden flooring-beams were laid on the flanches, and screwed down by 1-inch screw bolts. The stairs of the T head are supported in iron frames resting on the two pieces fixed to the flanches on the columns. Oak steps are laid on the flanches of these frames, and screwed down with $\frac{5}{8}$-inch counter-sunk bolts and screws. The bottom and top of these iron frames are secured to their respective bearing-timbers, and to the side of the columns.

The promenade and T head are surrounded by a cast-iron fence, the small pillars of which are half an inch in thickness, and the longer pedestals $\frac{5}{8}$ths of an inch, all having flanches at the bottom for fixing them perpendicularly and true, one with the other, in every direction, by means of $\frac{3}{4}$-inch screw bolts and nuts. Through the small pillars are cast 3 holes 3 inches deep, and $\frac{5}{8}$ths of an inch wide, to admit the horizontal fence-bars of wrought-iron to

pass at the back of the diagonal braces, which, as well as the handrail, are of Memel timber. The awnings at the east and west ends of the T heads, are supported by six ornamental columns, each an entire casting $\frac{7}{8}$ths of an inch thick, and screwed down by eight $1\frac{1}{8}$-inch screw bolts and nuts.

The bearing-timbers forming part of the cornice round the exterior and interior of the T head, and on each side of the promenade, over the arches to the quay wall, are 6 inches deep and 12 inches wide. These timbers are secured to the cast-iron work with $\frac{7}{8}$-inch screw bolts and nuts passing through the cast-iron flanches for fixing the cornice. From the centre of the T head to the termination of the promenade, run longitudinally five bearing-timbers, secured nine inches into the quay wall, and to their respective flanches by $\frac{7}{8}$ screw bolts and nuts. The whole area of the promenade, and of the T head, is then covered with 4-inch Memel plank, in proper lengths for breaking joint; a plank at every ten feet apart extending the entire width of the promenade.

The framing of the roofs of the awnings and the turrets are shewn in the drawing (Plate VI.); the acroteria forming the ornaments or tiles at the top of the cornice are of cast-iron, and firmly screwed to the cornice. The roofs of the awnings and turrets are covered with copper 16 oz. to the foot.

Lighthouse. Long after the plans had been matured, and the castings made, it was determined to have a cast-iron column to exhibit a night-light (Plate III.) This column is situated at the centre of the pier on the line of juncture of the promenade with the T head. It is 30 feet in height, 3 feet in diameter at the bottom, and 2 feet 6 inches diameter at the top. It has a staircase in the centre, and was cast in three pieces. The lantern is glazed with red glass, and lighted by gas.

DESCRIPTION OF THE PLATES.

Plate I.

A general plan of the approaches and site previous to the erection of the pier, shewing the town-wall and quay; the stairs and bridge, or jetty, leading down to the river; quoting the distances from the town and quay wall, and the depths of water at different points, at low-water of spring-tides.

Sections on different lines of the plan, shewing the levels and depths of soil and water with reference to low-water spring-tides.

Scales, shewing the rise of the tide at Gravesend pier, and at the entrance lock of the Thames and Medway Canal.

A general plan of the foundations of the new Pier; shewing the foundations next to the quay-wall, and the bolt-holes for fastening the frame-work to carry the steps leading from the watermen's causeway to the quay. Also the seven other foundations for the iron framings which support the causeway. On the first foundation wall is shewn in section the iron framing; the others only shew the bolt-holes. The foundations of the columns of the promenade. The frame-work for fixing the piles, the guide-plates being shewn on one half, and the piles on the other half. The longitudinal section of the foundations, and a section of the columns and piles, and the temporary frame-work.

Plate II.

Side elevation and general plan of the pier.

Side and end elevation, and plan and dimensions of the half ribs which form the tie with the quay-wall. This is bolted down to the foundation-plate in the quay-wall by vertical and horizontal bolts (*see* p. 253), the former of which is shewn at the end of the flanch in the elevation.

Elevation of the half rib, forming part of T head. At A, B, C, are the distance pieces, the details of which are given in Plate V. At A is shewn the union of two ribs at the centre, the bolts passing through the transverse pieces. At D is the end of the deep-bearing beam of the T head.

Elevation of a half rib and column of the promenade, shewing the bolts of the distance pieces.

Plate III.

The river front.

Plate IV.

Details of the columns, piles, guide plates and adjusting plates, with the dimensions.

Details of the deep-bearing beams, forming the T head. The situation of these in the plan of the structure is shewn by A, B, C, in Plate VI.

Details of the steps at T head. The columns on the plan of the steps, marked *e*, *d*, *g*, *k*, are those referred to by the same letters in Plates V. and VI.

Plate V.

Details of the distance pieces, the situations of which are shewn by the letters A, B, C, Plate II.

Details of caps of columns and deep-bearing beams of T head. The columns *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *k*, are those shewn by the same letters in the plan on Plate VI. The column *k* being the lamp column, Plate III. The column *i* is not shewn on the plan Plate VI. but is one of the columns of the promenade, these being all alike. The columns in the other half of the T head occupy the same relative positions as those here described.

Plate VI.

Elevation of the awnings, or pavilions and turrets, and details of the woodwork of the superstructure.

Plan of T head; one half shewing the manner of fastening the iron bearing beams to the caps of the columns, and the other half the plan of the timbers of the floor resting upon the iron beams. A, B, C, are the positions of the bearing-pieces, the details of which are shewn on Plate IV.; and *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *k*, the positions of the columns, the details of which are given in Plate V.

X.—*On the Action of Steam in Cornish Single Pumping Engines.*

BY JOSIAH PARKES, M. INST. C. E.

Read June 16th, 1840.

Introduction.

It is generally believed and admitted by engineers that some economy is derived from working steam expansively; that is, by introducing the steam upon the piston of an engine at a pressure exceeding the force of the resistance opposed to it—by stopping its further entrance into the cylinder at some portion of the stroke—and, finally, by allowing it to expand to the end of the stroke, when it quits the cylinder at a pressure less than that of the resistance.

By the term, economy, is meant the increase in the quantity of action, which results on the expenditure of a given weight of water as steam, thus applied, compared with the quantity of action which results on the same weight of water as steam being used unexpansively; that is, at a pressure throughout the stroke forming a constant equilibrium with the resistance.

Notwithstanding, however, the length of time which has elapsed since the principle of expansive action has been practically applied, the amount of economy obtained therefrom still continues to be disputed, and its advantage even altogether denied. The opinion still finds adherents that the effect of any given weight of water as steam will be identical, whether it be introduced into the cylinder, gradually, at a pressure counterpoising the resistance, and following up the piston with the same pressure till the stroke be completed; or, whether it be introduced at a higher elasticity during only a portion of the stroke, and suffered to dilate to the end of it. This doctrine is equivalent to the assumption that, at the termination of the piston's course, the cylinder, on both principles, must be filled with steam of the pressure of the resistance. If such were the case, the quantity of action resulting from equal weights of water as steam throughout a stroke, would necessarily be the same on both systems, and no economy could result from the expansive principle.

This, then, is a question of fact, determinable by ascertaining the elastic force of the steam when it quits a cylinder, and by comparing that force with the absolute resistance overcome.

Cornish System.* The Cornish engineers have constructed the largest sized, and greatest number of engines on the expansive principle. They early adopted a system which, in their belief, carried with it practical proofs of its advantage. They augmented the pressure of the steam in the boiler; they introduced this steam into the cylinder, by opening the admission valve very suddenly, and fully, instead of gradually; they intercepted the steam's further entrance, when but a small portion of the stroke was performed; they used cylinders of large capacity, and caused the steam to develope a considerable part of its action on the piston, whilst expanding from its initial, to its terminal elasticity.

Perceiving an increased effect to be the consequence of these methods and contrivances, they have apparently cared little about the theory of the steam's action, or explanation of first causes. They have contented themselves, during a long series of years, with referring—as a sufficient refutation of the opinions of their antagonists—to the evidence of the economy of expansive action, afforded by the relative performance of equal measures, or weights of coal consumed, according as the steam was applied more or less expansively, or unexpansively.

Disbelief in the statements of these practical men would have long since vanished, if the consumption of water which entered the cylinders as steam, had been regularly used, or even occasionally resorted to, instead of fuel, as the standard of the engine's performance; for, whilst the construction of the engine, and application of the power were modified, the evaporative product from a given weight of coal was increased: the machinery of pumps and pitwork was also constructed on more effective principles. Thus, the progressively increasing performance of the Cornish engine, though in fact arising from various sources, was referred, on the one hand, solely to the use of expansive steam, and, on the other, set down as unworthy of implicit faith.

The general truth of the statements of Cornish duty has always appeared to me indisputable, nor could I doubt the numerous facts reported by other engineers, corroborative, in a greater or less degree, of the economy attributable to the expansive system. It seemed to me that any experienced practical man

* An excellent engraving of Davey's engine, Consolidated Mines, from drawings by Mr. John Hocking, appeared in the "Annales des Mines," vol. v. 1834, with a clear description of the structure of its several parts by M. Combes, Ingenieur des Mines. This is the only published account of the action of a modern Cornish pumping engine with which I am acquainted.

was capable of determining the head of water against which an engine worked, and of reducing it to the terms employed in practice. Nevertheless, I have been at a loss to account for the greatest assigned performances of these engines. I gave full credence, after a patient investigation of the experiments, to the fact of the stated effects; but was unable to trace them, in the highest range of duties quoted, solely to the increased quantity of action obtainable from steam, expanded only to the extent assigned by the reporters of the experiments.

Data required for investigating the steam's action.

Until very recently, the data necessary for the satisfactory determination of Cornish economy remained unknown, or were not collected. We possessed no record of exact experiments made at any one expansive engine, whether single or double acting, on the quantity of water, in the shape of steam, which had passed through the cylinder during an observed number of strokes; whence, alone, the relation which the steam's mean force bore to an ascertained resistance could accurately be deduced. A knowledge of the duty, or effective performance of an engine by a given weight of coal is, obviously, no criterion of the expenditure of power; it sheds no light on the steam's action in the cylinder; nor do the periodical returns of Cornish duty, however practically useful, afford data for determining the absolute resistance overcome by the steam.

Huel Towan engine.

Mr. Henwood's communication (Trans. Inst. C. E. vol. ii.) contains, I believe, the first published observations on the quantity of water as steam consumed by a Cornish single pumping engine. These were made on the Huel Towan engine. The fact was elicited that the steam's elastic force in the cylinder was far from being uniform during the period of its admission, but that it was in a state of expansion from nearly the beginning to the end of the stroke. This result is manifested by the indicator diagram obtained from that engine.* A similar action was denoted by the instrument in several other engines to which it was applied.

He noticed the fact of the steam's elasticity at the end of the working stroke being unable to sustain the column of water; and recorded numerous quantities, and observations, of much interest and assistance in the present research.

With respect to the determination of the load of water, he twice measured the height of the lifts himself, and twice ascertained the whole water delivered

* This previously unnoticed phenomenon is referred by Mr. Henwood (see his paper, p. 50) to the diminishing elasticity in the boiler (see Note to p. 272, also my last paper, Trans. Inst. C. E. vol. iii. p. 61.)

by the pumps of the Huel Towan engine to correspond with the calculated quantity within less than 8 per cent.*

Fowey Consols engine. The next engine for which the consumption of water as steam is satisfactorily ascertained is the one at the Fowey Consols, which accomplished the greatest duty yet reported.† The evaporation was not determined by the committee who conducted the trial, but, subsequently, by Mr. West, the maker of the engine, in the manner stated in my last paper (pp. 62, 63).

Holmbush engine. Mr. Wicksteed's experiment on the weight of water actually delivered by the pumps of the Holmbush engine, confirmed Mr. Henwood's accuracy at Huel Towan; and as the duty at Holmbush‡ nearly approached that at Fowey Consols, it afforded additional testimony to the authenticity of the stated performance of the latter engine. Mr. Wicksteed, unfortunately, did not ascertain the expenditure of water as steam during his experiment, a datum which I have supplied, for the purposes of this investigation, by assuming a consumption based on an equal evaporation to that effected at Fowey Consols and Huel Towan, viz., 10·5 lbs. of water converted into steam by 1 lb. of coal.

Analysis.

The Huel Towan, Holmbush, and Fowey Consols experiments analysed.

The preceding results form the subject of the present analysis, undertaken in the hope of discovering, from the known consumption of water as steam, the whole quantity of action developed by it—its action, had it been used unexpansively—the precise value of its expansive action—and the correspondence between the appreciable power and effect, in each engine. The loads of water, dimensions of the engines, number of strokes, consumption of water as steam, and other necessary data are registered in the annexed table.

The absolute effect of the steam, in these examples, is not so readily determinable as the absolute power exerted by it. The rods suspended to the beam of a Cornish engine perform the pumping or return stroke. Their

* The duty effected during Mr. Henwood's experiments in 1831 was 81·389 millions. On a shorter trial in 1830 the duty was reported as 92·33 millions; and in 1828, during a trial of 26 hours, 87·20 millions. The near correspondence between the calculated and actual quantity of water discharged by this engine, has also been ascertained by other experimenters.

† 125·089 millions. For an account of the trial of this engine see "Lean's Historical Statement of the Steam Engines in Cornwall," p. 98.

‡ 117·906 millions.—See Mr. Wicksteed's paper, Trans. Inst. C. E. vol. ii.

weight must, therefore, be superior to the column of water on which they act, by the amounts necessary to overcome the friction of the water in the pipes—to displace it at the velocity of the stroke*—to overcome the pitwork friction arising from the pumps, rod-guides,† balance beams, &c.—and the friction of the engine itself.

This mass of matter constitutes the solid weight raised by the engine, but does not equal the absolute resistance opposed to the steam. In bringing back this weight, during the working, or descending stroke of the piston, the steam has also to overcome the whole amount of engine and pitwork friction, together with the resistance from imperfect vacuum beneath the piston.

Determination of the absolute resistance.

The absolute resistance, therefore, consists of the weight which performs the return stroke, plus the value of engine and pitwork friction, and of the elasticity of the uncondensed steam. The method of correctly ascertaining this resistance, which is the steam's effect, will be shewn in the sequel (p. 280).

Of the several component portions of the total resistance, the water load can alone be termed a positively ascertained quantity. This is found by the method of taking the duty, and amounts for each engine to the following sum:

* Mr. Henwood's paper supplies data for determining the velocity of the water in the pipes at Huel Towan. The return stroke was performed in 4·8 seconds, and the length of stroke in the pumps being 8 feet, the discharge was effected at the rate of 1·666 feet per second, or 1·13 miles per hour.

The entire column of water displaced at that velocity was 899·07 feet in height, and the mean diameter of the pumps 14·625 inches. The pipes are usually larger than the pumps, but the friction of the water is often greatly increased in the lifts by incrustation from mineral deposits. Mr. Henwood informed me that he has seen pipes of 9-inch bore diminished to 5 inches from this cause.

Mr. Moyle, an experienced Cornish engineer, has also informed me that he has known pipes of 14 inches bore reduced to 12 inches by a coating which consisted chiefly of oxide of iron, mixed with a little copper, and carbonate of lime.

I am not aware of any experiments made with the view of determining the head or pressure requisite to force water through pipes at different velocities, circumstanced as mining pumps are. It is, however, manifest that in order to give a velocity of 1·13 miles per hour to a column of water 900 feet high, the pump rods, besides being heavy enough to balance the column, and overcome friction of all kinds, must also have a preponderance sufficient to drive the water at the required velocity.

Mr. George Rennie, in his Report on Hydraulics, cites some experiments by Mr. Tierney Clarke to shew that "the friction and resistance of the pipes to the free motion of the water through them have been found to be between one-fourth and one-fifth of the total height of the column."—*Fourth Report of the British Association for the Advancement of Science*, p. 416. This seems a large consumption of power for water friction, but the velocity of discharge is not stated, which is probably in most cases greater in a waterwork than in a mine engine.

† "The rods of the Wheal Towan engine varied from 14 to 12 inches square, and were kept in their places by 13 sets of guides, which exposed a surface of about 53·5 square feet."—Henwood.

Per square inch on the piston. lbs.	
*11·01	at Huel Towan.
9·98	at Holmbush.
9·26	at Fowey Consols.

The value of the various frictions being unascertained, I am compelled, in the outset of the investigation, to assume an equivalent, and have estimated them to amount to 5·75 lbs. per square inch on the Huel Towan engine. To this has to be added the resistance from imperfect vacuum beneath the piston, which I assume as 1·25 lb. per square inch; forming a total of 7 lbs. per square inch in addition to the water load.

As the other two engines were not burthened with so high a column of water, I have assumed about 6 lbs. per square inch for these additional resistances.

I consider these amounts to be under rather than overrated, and shall, subsequently, offer evidence, amounting to demonstration, that the steam had to overcome, in the Huel Towan engine, a somewhat greater resistance than that now assigned. It will appear, too, that though the quantities set down as the value of these surplus resistances can be considered only as approximations, they will not affect the accuracy of the principal facts developed by the analysis.

The absolute resistance thus assumed to be overcome by the steam is—

Per square inch on the piston. lbs.	
18·01	at Huel Towan.
†16·00	at Holmbush.
15·25	at Fowey Consols.

* Erroneously printed 10·2 lbs. in Mr. Henwood's paper.

† Mr. Wicksteed (see his communication previously quoted, p. 64) has estimated the "friction of the machinery" of the Holmbush engine as equal to 7·75 lbs., and the resistance from imperfect vacuum as 1·50 lb., making 9·25 lbs. per square inch on the piston; which, added to the water load, would bring the absolute resistance of this engine to 19·23 lbs. It will, hereafter, be seen (note p. 275) that an erroneous datum of the steam's density in the cylinder, during its admission, led to this estimate.

The same author cites "the friction of a waterwork pumping engine as about 5·75 lbs. per square inch," but, whether this sum includes the friction of the water in the pipes, or, whether it is only "the friction of the machinery," is not stated. These amounts should be separately determined by experimenters.

At its entrance into the cylinder, the steam's pressure exceeds that of the force opposed to it, or the engine would not stir; at the termination of the stroke, its pressure is reduced below that of the resistance, as already found; nevertheless, the sum of the forces operating throughout the stroke must be sufficient to carry the piston to the end of its course, or it could not arrive there.

The mass of matter set in motion by the steam's initial force on the piston, acts the part of a fly-wheel. It first absorbs the excess of power over the resistance, and then faithfully discharges it, by assisting to continue the engine's motion during the steam's expansion, and consequent diminishing force, below the pressure of the resistance. But this mass of matter possesses no power-creating virtue; it merely husbands at one period of the stroke, and restores at another, that force which has been given to it; it exerts no power of its own.

From a due consideration of the origin and destruction of the momentum arising from this source, it is evident that the effect of a given weight of water as steam, consumed during a stroke, will be the same, whether that steam be regarded as having been all enclosed between the piston and cylinder cover, before the piston were permitted to move, when it would expand nearly uniformly from the beginning to the end of the stroke; or, whether it be considered as having been admitted during a portion of the stroke, at some pressure greater than the resistance, and then expanded through the remainder of the stroke.

But, the value of expansion consists, virtually, in the quantity of action derived from the steam, after it forms an equilibrium with the resistance. This, then, is the first point to be ascertained. By investigating the steam's action on this plan, that is, by tracing it, first, through the space of the cylinder, or portion of the stroke performed, when it would barely balance the resistance; and, secondly, through the space during which it suffered expansion below that pressure, a true measure of the respective and total quantities of action developed by it, unexpansively and expansively, will be obtained. This is the only analytical process which can detect the quantities sought, for we are uninformed either of the steam's mean elastic force during the term of its admission into the cylinder, or, of the exact period of the stroke at which its further admission was stopped.

The portion of the stroke at which the steam's pressure equalled that of the

resistance, is discoverable by the method of the volumes; for, the volume of water composing the steam within the cylinder being known, the volume which the steam must occupy, in order to balance the resistance, is also known; and the increment of the stroke, which gives the necessary capacity to the cylinder, is found from the ratios which the volumes of steam and water must bear to each other for the required pressure.

It results that, when the piston of the Huel Towan engine had passed through 50·70 out of 120 inches (Appendix, 2), which was its total length of stroke, the steam's elastic force and the resistance counterpoised each other. The equilibrium was established in the Holmbush engine at 32·26, in a stroke of 109 inches; and in the Fowey Consols at 33·78, in a stroke of 124 inches.

It is thus manifest that, at these respective portions of the strokes of the three engines, the steam, if used unexpansively—that is, if admitted gradually, at the pressure of the resistance—would have done its utmost, and the engines would have come to rest; yet there remained 69·30 inches in the first engine, 76·73 inches in the second, and 90·22 inches in the third, of the entire stroke to be accomplished. These last portions were performed during the steam's expansion from an elastic force equal to the resistance, to the elastic force it possessed at the end of the stroke.

It is through these portions of the stroke that the expanding steam is assisted by the discharge of the momentum transferred to the mass by the excess of the steam's force over the resistance, during the period intervening between the instant of its entering the cylinder, and the instant when its elasticity falls below the resistance. The value of this temporary excess of force forms part of that attributed to the expanding steam, as no expansion could have taken place below the line of equilibrium between the acting and opposing forces, but for the momentum husbanded and restored. (App. 10, *Obs.*)

The spaces through which the pistons were urged by virtue of the action of an elastic force equal to the absolute resistance, and less than the absolute resistance, being thus separately ascertained, the quantities of action derived from each are shewn by the dynamic effect produced—that is, by the number of pounds raised one foot; and this result is obtained by multiplying the pressure on the piston, by the number of feet through which it travelled. The pressure used in computing the effect during the expansive portion of the stroke is the mean of the steam's elasticity at the commencement of useful expansion, and at the termination of the stroke.

Absolute power of the steam compared with the absolute resistance.

By adding together these unexpansive and expansive effects we obtain the whole effort of the steam, in terms of weight raised one foot; which, being divided by the length of stroke in feet, gives the mean load of steam upon the piston throughout the stroke; and this sum, divided by the area of the piston, gives the mean pressure in pounds per square inch exerted by the steam throughout the stroke.

A precise comparison is thus arrived at between the pressure of the steam on the pistons, and that of the resistance opposed to it: and it results that the steam possessed a mean force, reckoned throughout the stroke, of—

Per square inch on the piston. lbs.	
14·85	at Huel Towan. (App. 8.)
11·47	at Holmbush.
11·13	at Fowey Consols.

Whereas, as before shewn, the resistance to be overcome required—

Per square inch on the piston. lbs.	
18·01	at Huel Towan,
16·00	at Holmbush.
15·25	at Fowey Consols.

Which exhibits a deficiency of power throughout the stroke, of—

Per square inch on the piston. lbs.	
3·16	at Huel Towan. (App. 9.)
4·53	at Holmbush.
4·12	at Fowey Consols.

Deficiency of Power.

The results obtained at this stage of the investigation demonstrate that the mean force of the steam, in the cases under review, was unable to overcome the resistance; they present to us the seeming paradox that the lighter weight has raised the heavier one. But, we know that a force equivalent to the resistance must have operated throughout the stroke, or motion would not have continued.

How is it then that, in these instances, the steam's force, which may be termed the lighter weight, has actually counterbalanced the resistance and

raised the heavier weight? In this consists the seeming paradox. To attempt its explanation by saying that such an effect can be produced by the steam's mere expansive action, is to use words of no meaning; for steam, in the act of dilatation, diminishes in elastic force; and the absolute effort, exerted by virtue of the steam's initial pressure, and subsequent expansion, can only amount to that of its mean pressure reckoned throughout the stroke; and this has already been proved to be far unequal to the effect. It would be necessary to prove that steam gained, instead of lost elastic force, during the act of dilatation, in order to account for the entire performance of the engine, in these instances, by expansion alone.

It was the glimpse I had obtained of this deficiency of power—of this apparent discrepancy between cause and effect—which induced me to desist, when writing my last paper, from any attempt to illustrate the action of steam in Cornish engines. The materials for the analysis were therein given, and the data now employed furnished to those who might be disposed to investigate them. The subject seemed to me worthy of an examination distinct from the matters then treated of, and to require for its elucidation much patient thought and research; for, unless a power could be discovered to have acted commensurate with the effect produced, no other alternatives presented themselves to my mind, than a rejection of the data as erroneous, or the belief that the received theory of steam was insufficient to explain its action.

With regard to the data of power, there seemed to me much greater reason to suspect some excess in the consumption of water as steam assigned by the experimenters, than the contrary; for the evaporation cited from a given weight of coal is the greatest on record. The steam is also assumed to have been strictly pure; every particle of the water evaporated is presumed to have acted as force; no loss of any kind is allowed for. Doubt could not be entertained as to the dimensions of the engines, nor as to the number of strokes made by them during the observations; neither could suspicion of any material error attach to the elasticities deduced from the relative volumes of steam and water consumed.

With respect to the approximate datum of absolute resistance, I felt assured —from a consideration of the peculiar circumstances under which these engines work—from analogy—and from certain phenomena, to be hereafter explained —that the sum assumed as the basis of calculation was below, rather than above, the amount of absolute resistance opposed to the steam.

The dilemma was perplexing, but I felt animated in the further investigation of this mysterious discordance between the appreciable power and effect, by the reflection that, like the celebrated hydrostatic paradox, the problem might admit of solution; and that some new light might be shed upon the science of the engine. I was thus finally led to doubt the sufficiency of the ordinary theory to account for the whole power exerted by steam, and was induced carefully to examine its action under the peculiar circumstances of its application to Cornish engines, in the hope of discovering the force which made up the complement of power necessarily exerted in overcoming the resistance.

I will now proceed to develope the opinion I have formed on this subject, together with the facts and arguments in support of it.

Theory of the Steam's Action.

Steam, in its action on the piston of an engine, has hitherto been considered as simply exerting elastic force.

Percussive Action of Steam.

Steam, however, possesses another important property, equally inherent in its nature with pressure and expansibility. This property is the velocity and consequent momentum due to steam of high elasticity; a force which comes into play under the peculiar conditions of a Cornish engine. The velocity of steam, in passing from a dense into a rarer medium, is immense; and the momentum of this steam must be very considerable. On the sudden and free communication effected between the cylinder and boiler of a Cornish engine, the steam in the cylinder receives an instantaneous action, proportionate, in amount, to the velocity of the entering steam; and this action, by the property of fluids, is transmitted to the surface of the piston. This action, thus transmitted to the piston, and due to the communication suddenly established between the highly elastic steam in the boiler, and the steam in the cylinder, may be likened, I conceive with great propriety, to the force of percussion; by which term I propose to distinguish it from the action of the steam's simple elastic force.*

It will accordingly be found, as I proceed in the investigation, and reveal

* The force which I have expressed by the term percussion will, perhaps, be rendered clearer to some readers by the use of a comparison. The pile-driving machine illustrates the action of steam when suddenly and fully let upon a piston, as it is in the Cornish engine. The monkey strikes the pile

the quantity of action derived from this unsuspected source, that the most economical of the three engines is the one in which this action has been most fully and dexterously applied. It will also distinctly appear that the degree of useful expansion mainly depends on the amount of percussive action realized, not on the degree of the steam's nominal expansion, as usually calculated from the period of the stroke at which the admission valve is closed.

Determination of the quantity of Percussive Action.

The dynamic effect, or quantity of action, due to percussion, is discoverable, and assignable for each example. It may be measured by the length of stroke performed by its sole influence—by its separate value throughout the expansive portion of the stroke—or by its value throughout the entire stroke.

The mean elastic force of the steam, during its expansion in the cylinder, from the line of its equilibrium with the resistance, to the end of the stroke, was—

Per square inch on the piston. lbs.	
12·55	at Huel Towan. (App. 6.)
10·30	at Holmbush.
9·60	at Fowey Consols.

During, therefore, this usefully expansive portion of the stroke, the steam's simple elastic force, compared with the resistance, was in deficiency by the following amount, viz.—

Per square inch on the piston. lbs.		Expansive stroke. Inches.	
5·46	through	69·30	at Huel Towan. (App. 9.)
5·70	„	76·73	at Holmbush.
5·65	„	90·22	at Fowey Consols.

head with a force as much greater than that of its simple weight as is due to its velocity at the instant of impact.

The force resulting from the motion of fluids, as in the hydraulic ram, illustrates my meaning equally with the pile-driving machine.

I have adopted the term percussion, though usually applied only to solids, as the most appropriate and significant I can use; but, though assuming identity of character in the force, I am far from intending to convey the idea of equality in the effect derived from the motion either of solids, or fluids, and that of an æriform elastic fluid like steam.

It forms no part of my task to investigate the abstract question of the quantity of this species of force to be obtained from steam; my present purpose is confined to the determination of the effect attributable to it in the three engines subjected to analysis.

These sums represent the amount of aid given to the expanding steam by the discharge of the momentum originally impressed on the mass of pump rods, &c. by the steam's percussive action.

We have now the means of finding the space through which the pistons were carried by the sole influence of the percussive action; for, by the foregoing method of treating the subject, the elastic force of the unexpansive and expansive steam—the spaces through which these forces acted—and the deficiency of the expansive force to complete the stroke, are known. The influence of the percussive action, therefore, is producible in terms of the portion of the stroke performed by it; for the proportion of what I have termed the expansive stroke, really effected by the steam which entered the cylinder, is as the ratio between the whole pressure required to overcome the resistance, and the mean pressure of the expanded steam: and it results that the deficiency in the steam's expansive force to complete the stroke was equivalent to—

Inches of the stroke.	
21·01	at Huel Towan. (App. 10.)
27·33	at Holmbush.
33·43	at Fowey Consols.

The pistons were driven through these spaces by the force of percussive action alone, for the steam's simple elastic force was capable only of carrying the entire load through—

Inches of the stroke.		Total length of stroke. Inches.
98·99	at Huel Towan, out of	120. (App. 10.)
71·67	at Holmbush, out of	109.
90·57	at Fowey Consols, out of	124.

This final result may appear startling, and I confess that when it flashed across my mind, as a truth, that some portion of the stroke must have been performed without any expenditure of steam, I was myself startled, and set about investigating the problem with great doubts of being able to detect, and assign the quantity of action which it seemed to me could only be attributed to the impact on the piston, on the sudden opening of the admission valve.

Conditions under which a Cornish engine works.

But let us consider under what circumstances the engine is when steam is admitted on its piston. A complete stroke has been performed —the engine has been brought to rest by the resistance of a cushion of steam

imprisoned between the piston and cylinder cover—a vacuum is formed beneath the piston—the pressure of this cushion of steam, together with that of the column of water against the plungers, then sustains the entire weight which operated to perform the return stroke. In this state of things, a communication is suddenly and freely opened between the cylinder, and boiler, the elasticity of the steam in the latter being five or six times greater than in the former. The piston is free to move with a comparatively slight increase of insistent pressure, and must necessarily feel, for an instant, percussion; it is impelled by a blow inflicted upon it by the momentum of the entering steam. The piston, therefore, is started by this sudden action, and there is no equal force to follow, for the steam's further influx into the cylinder is not only retarded, but its force is diminished in intensity by its passage through the throttle-valve, and by other causes.*

Percussive action detected by the Indicator Diagram.

Mr. Henwood's indicator diagram distinctly exhibits the action I have just explained. Had not this transcript of the piston's movements, and of the steam's elasticity, been taken at the same time that its consumption was ascertained, I might have failed to convince either myself or others of the conformity of this theory with truth. That the piston has received an almost instantaneous impulse, and greater than the force of the pursuing steam, is made evident by its retiring faster than the steam can follow with equal force. The diagram assures us that the piston has scarcely moved before the steam begins to dilate within the cylinder; it shews that a void is more quickly created by the piston's velocity, than the continually entering steam can fill up at an uniform density. Were not this the fact, the diagram would exhibit an uniform pressure of steam from the first instant of its admission till the instant of cutting it off; for it must be borne in mind that the admission valve remains wide open during that period, and that if the piston moved only in obedience to the simple elastic force of the steam, its velocity would be uniform during the entire interval of communication between the cylinder and boiler.

Mr. Henwood has given data which will assist in making some approxi-

* The throttle-valve is the *governor* of the engines under consideration. Its position is between the steam-valve and boiler. The steam-valve, once set, works for weeks and months without any alteration being made in its lift, or in the duration of its opening. The throttle-valve requires and receives continual adjustment; it is always open, but its aperture is so regulated as to *wire-draw* the steam more or less as the pressure rises or falls in the boiler. By this arrangement the steam-valve determines the quantity, and the throttle-valve the elasticity of the steam admitted on the piston. In other Cornish engines the throttle-valve is not used, and the engine is regulated by the lift, and duration of the opening of the steam-valve.

mation to the velocity of the steam's influx into the cylinder of the Huel Towan engine. The duration of the working stroke was 1·6 second; the steam's influx was stopped at about 0·22 of the stroke, which, on the supposition of an uniform rate of motion by the piston, was effected in about one-third of a second. In this time a mass of steam forming 76 cubic feet, and weighing 6·64 lbs., entered the cylinder. The area of the valve was 50·26 square inches, and that of the cylinder, deducting the piston-rod, 4988·08 square inches. These data give a mean velocity to the current of steam, through the valve, of 600 feet per second, or 400 miles per hour.

But the steam's initial velocity, on the opening of the valve, must have greatly exceeded this mean rate. The indicator diagram shews that, from the instant the piston had travelled 6 inches, or had made $\frac{1}{20}$th part of the stroke, the steam already within the cylinder, as well as the steam still entering it, underwent rapid expansion; proving that the piston moved faster than the steam continued to get into the cylinder, and maintain an elasticity within it equal to that it possessed at 6 inches of the stroke. The steam, in consequence, suffered expansion, as shewn by the diagram, during 19 out of 20 parts of the piston's course, though it was admitted through an unvarying aperture, during 4 out of 20 parts, and the piston's velocity must have diminished with the steam's attenuation* (*see* Plate).

Phenomena illustrative of percussive action.

Mr. Henwood's paper affords me another illustration of the truth of the theory I am attempting to develope. It is derived from his observation that, as the steam's pressure increased in the boilers, the temperature of the water discharged by the air-pump diminished. He remarks, "As the pressure of the steam in the boilers increased, the temperature of the hot well

* The steam's rapid expansion during its influx into the cylinder cannot have been caused by a diminishing elasticity in the boilers. The steam reservoir contained 700 cubic feet at a pressure of 64·1 lbs.; the quantity abstracted per stroke being 76 cubic feet at a mean pressure of 24·75 lbs. per square inch. Allowing for the difference in density, the quantity introduced into the cylinder amounted only to $\frac{1}{10}$th of the mass of steam in the boilers; whilst the variation in its elastic force in the cylinder, during admission, was $\frac{1}{3}$th. The boilers, also, contained 1080 cubic feet of water of the temperature of the steam, which would instantly supply the volume abstracted at a very slight diminution of elasticity.

On the supposition that the first six inches were performed in the mean time of the stroke, and whilst the valve was being opened by the cataract, that operation consumed about 0·080 second.

The piston's velocity during the working stroke was at the rate of 375 feet per minute. It would be curious and instructive to ascertain the periods of time in which equal increments of a working stroke are performed. This might be accomplished, and the steam's elasticity taken, simultaneously. Similar observations should be made during the return stroke, which in this case was effected at the rate of 100 feet per minute by the plungers.

declined, so that by observing the alteration in one, that of the other could be predicted with great certainty."

Now let us examine this fact, and its bearing on the subject. An increasing elasticity in the boiler would cause the engine to make a longer stroke, were the steam permitted to enter the cylinder at that increased force; but its elasticity is reduced by diminishing the aperture of the throttle, or governing valve. This constitutes the practical regulation of the engine's stroke, for the admission valve remains unaltered, and fully open, during a constant portion of the stroke. The quantity of water injected into the condenser was also constant during these observations; yet the phenomenon is presented of a diminished temperature communicated to that water by a cylinder full of steam, when the elasticity, and consequently the temperature of the steam within the boiler is the greatest!

The explanation is obvious. A less weight of water as steam suffices to perform the stroke, when the steam's elasticity increases in the boiler; for the greater the difference between the elastic force in the boiler, and that in the cylinder, at the commencement of the stroke, the greater will be the percussive action transmitted to the piston; consequently, steam of a less density will be required to complete the stroke. The regulation of the throttle-valve effects that object, and since a less weight of water as steam is admitted into the cylinder, less heat passes from the cylinder into the condenser.

During the observations recorded on the Huel Towan engine, the range of the steam's pressure in the boilers varied from $77\frac{1}{4}$ to $47\frac{1}{4}$ lbs., or 37 lbs. per square inch; and its temperature, consequently, from 311° to 279°, or 32°. The temperature of the water discharged by the air-pump varied, inversely, through a range of from 90° to $100\frac{1}{2}$°, difference $9\frac{1}{2}$°.* The steam's influx into the cylinder was intercepted at precisely the same portion of the stroke throughout the experiments, so that its volume, when cut off, and its subsequent expansion, were constant quantities. But, it is certain that the steam quitted the cylinder in a more attenuated state when its elasticity was greatest in the boiler, for a given volume of steam is seen to have communicated a less

* Henwood, page 55, Table 2. Careful observations on the temperature and quantity of water injected into, and discharged from the condenser of these engines, made concurrently with exact observations on the steam's elastic force in the boiler, and its force in the cylinder at the end of the working stroke, would elicit many highly useful and instructive facts.

amount of heat to a given volume of condensing water. Had the admission valve been closed earlier, as the steam's elastic force increased in the boiler, the phenomenon in question would have been attributed to the effect of a greater quantity of expansive action; but no greater nominal expansion was given. Be it observed, that the only physical change in the condition of the engine, during the experiments, was the variation in the steam's elastic force in the boiler; and the only mechanical change was that of the greater or less aperture given to the throttle-valve. Did not percussive action increase with the elasticity in the boiler, the throttle-valve would be so regulated as to administer steam of a constant force, and therefore, of a constant density, into the cylinder. But it is evident that an increased elasticity in the boiler necessitates, practically, as well as theoretically, a greater proportional contraction of the aperture; and thus it arises that steam of a gradually diminishing density exists in the cylinder, as its density and elastic force increase in the boiler.*

Other corroborative evidence of the truth of this theory might be adduced from the well-known fact in Cornwall, that the duty of an engine falls off, or more steam and fuel are required to perform a given effect, when, from the circumstance of leakiness, or deterioration in the boilers, it is found necessary to diminish the pressure within them; the steam being admitted during the same portion of the stroke, and, therefore, no change being made in the degree of nominal expansion.

Economy not dependent on the degree of the steam's nominal expansion.

Returning to the examples under review, it is found that the economy attained is inversely as the steam's terminal elasticity; or, what is the same thing, directly as its attenuation in the cylinder, at the end of the working stroke. This conclusion would be predicted from a perfect knowledge, or true theory of the steam's action; for a greater economy of steam in one engine over another, or in the same engine at different times, can arise only from having exhausted the steam admitted into the cylinder of a greater proportion of its absolute force, in one case, than in another. That such was the fact, in these three instances, is forcibly exemplified by placing the effect produced by equal weights of water, as steam, in juxta-position with the steam's elasticity at the end of the working stroke.

* This has reference only to what occurs in any given engine. It may happen from various causes that, of two engines, the one which has the greatest elastic force of steam in the boiler may realize the least amount of percussive action in the cylinder.—(*See* note, p. 285.)

Absolute effect in pounds raised 1 foot by 1 lb. of Steam. lbs.	Steam's elasticity at the end of the stroke per square inch on Piston. lbs.	
135253	7·30 at Huel Towan.	(App. 11.)
196123	4·66 at Holmbush.	
210332	3·95 at Fowey Consols.	

The degree of what I have termed useful expansion is truly measured by the ratio which the absolute resistance bore to the steam's elasticity, on the termination of the working stroke, which was as follows:—

Ratio of the absolute resistance to the Steam's elasticity at the end of the stroke.	
As 1 to 0·405 at Huel Towan.	(App. 9.)
As 1 to 0·288 at Holmbush.	
As 1 to 0·259 at Fowey Consols.	

It thus distinctly appears that the steam virtually suffered the greatest expansion, or attenuation, in the engine which performed the greatest effect, and, hence, its superior economy. But the admission of steam is reported to have been intercepted at periods which assign the least nominal expansion to the most economical of the three engines, viz.—

At about	$\frac{1}{5}$ or 0·22	of the stroke at	Huel Towan.
,,	$\frac{1}{6}$ or 0·166	,,	Holmbush.*
,,	$\frac{1}{4}$ or 0·25	,,	Fowey Consols.*

* The capacity of the Holmbush cylinder above the piston at one-sixth of the stroke would be 20·178 cubic feet, exclusive of the space and passages, and, if filled with steam at 30 + 14·71, or 44·71 lbs. per square inch, assumed by Mr. Wicksteed as the pressure on the piston from the instant of opening to the instant of closing the steam-valve, the constituent water of that volume of steam would have been 2·06 lbs., representing the quantity expended per stroke. This sum, multiplied by 672, the number of strokes made by the engine, and divided by 94 lbs., the weight of coal consumed during his experiment, would give an evaporative product of 14·72 lbs. per pound of coal!

Mr. West states that the steam entered the cylinder of the Fowey Consols engine at 27 + 14·71, or 41·71 lbs. per square inch. Had the steam maintained this pressure in the cylinder throughout one-fourth of the stroke, its constituent water would have been 8·56 lbs., which, multiplied by 6287, the number of strokes, and divided by 2256, the number of pounds of coal burnt during the experiment, would give an evaporation of 23·8 lbs. of water by 1 lb. of coal!

Fortunately, Mr. West ascertained the real evaporation to be 10·50 lbs. per pound of coal, and, it being known that no instrument was employed to measure the steam's elasticity in the cylinder, in either of these experiments, but that it was only an estimation, doubt does not extend to the observed facts which remain unimpeached. The indicator would have shewn—as in the case of the Huel Towan—that whatever might have been the pressure of the steam on the instant of opening the steam-valve, its pressure in the cylinder, on closing it, would be very different. The indicator diagrams

The nominal amount of expansion suffered by the steam is, therefore, no criterion of economy, which mainly arises from the intensity of the percussive action, for, upon its intensity depends a large quantity of the expansive action which can be brought into play; and upon the quantity of the combined action of these two forces depends the economy realized, compared with the effect of steam used unexpansively throughout a stroke.

Phenomenon of the engine's voluntary retrograde motion at the end of the working stroke: its indication of percussive action.

I have stated that the proof of a force having operated, distinct from the steam's continuous action, did not altogether depend on the precise accuracy of the value assumed for the absolute resistance. I will now exemplify my meaning, by adducing additional evidence of the insufficiency of the steam's simple elastic force to perform the work of these engines, which will also throw light on the nature of the force which makes up the complement of power.

In reflecting on the phenomena which ought to attend a deficiency of the steam's simple elastic force, if such existed, it occurred to me that practical testimony to the fact might be obtained, by ascertaining whether the piston would return back on the completion of the working stroke, if the equilibrium valve were kept closed. The inference seemed to me inevitable that a return stroke would commence against the steam above the piston, if the complementary force had overcome a greater portion of the absolute resistance, during the working stroke, than was equivalent to engine and pit-work friction. In other words, if on the termination of the working stroke, the column of water, and the steam's pressure upon the piston, were jointly inferior to the weight of the pump-rods, the latter would preponderate, and a retrograde movement of the engine take place, until, by the steam's compression, an equipoise were established.

I thought it not unlikely that so good an observer as Mr. Henwood might have noticed whether the engine would so return against the steam. I accordingly requested him to inform me if he had done so, and if not, to make the experiment by keeping the equilibrium valve closed, and note what would happen. His reply is as follows:—" In order to see the engine return against the steam above the piston, I removed the tappet from the plug-rod, and kept

shew that no one can predict the steam's elastic force in the cylinder from a given elasticity in the boiler, it being very variable throughout the period of its admission. It is evident, also, that the whole power in operation cannot be deduced from the steam's mean elastic force previous to cutting it off, either by the law of Boyle, or any other theory of expansion.

force. It is manifest that this complementary force must have been of a transient description; that its effort was momentary, not continuous; and that its exertion must have taken place at the commencement, not at any subsequent period, of the stroke; for, at no subsequent period, could such a force have been called into play. The transient character is precisely that of a momentum originally communicated to the mass of the engine; and whence can it have been derived, but from the instantaneous action transmitted to the piston, on effecting the sudden communication between the steam in the cylinder, and that in the boiler?

This important fact of the preponderance of the pump-rods over the two opposing forces of the column of water, and of the steam's pressure on the piston is, also, demonstrable; for, we are in possession of the amounts of those forces in the Huel Towan engine, and can compare them with the force of steam in the cushion which was requisite to bring the return stroke to a termination, and to counterpoise the entire weight of the descending mass, with the aid of the column of water.

The two resistances against which the engine went out, when observed by Mr. Henwood, were—

Water load per square inch. lbs.		Steam's force above the piston, at end of stroke. lbs. per sq. inch.		Imperfect vacuum. lb. per sq. in.		Per square inch. lbs.
* 10·52	+	7·30	−	1·25	=	16·57

The pressure of steam in the cushion was 10·7 lbs. per square inch,† but there existed beneath the piston, from imperfect vacuum, an elasticity (assumed) of 1·25 lb., leaving a net force of 9·45 lbs. per square inch above the piston. This sum, added to the pressure of the column of water acting against the four plungers, forms a total of 19·97 lbs. per square inch, as the counterpoise of the mass of the pump-rods. It is evident that the same mass could not be sustained by a less weight, or 16·57 lbs. per square inch. The engine, therefore, on the exhaustion of that force which I have termed complementary to the steam within the cylinder, must necessarily retrograde. At the end of the working stroke an excess of weight over the opposing forces of the column of water, and of the steam in the cylinder, is thus proved to have existed in the pump-

* One of the five pumps worked by this engine was a lifting pump, and its load raised by the working stroke; so that in *going out* the engine was resisted only by the columns of water of four pumps. Henwood, p. 57.

† Deduced from the indicator diagram.

Methods of ascertaining the absolute resistance, and its component portions.

By means of an indicator, or mercurial column placed on the cylinder cover, the following measures of resistance may be obtained.

1st. The absolute resistance during the working stroke. To find this quantity, in terms of pressure, it is only necessary to give so much steam above the piston, as to produce equilibration between its force and that opposed to it. When the piston acquires the slightest appreciable motion in descent, the counterpoise is complete. The value of imperfect vacuum must be ascertained, and deducted from the pressure above the piston.

2nd. By keeping the equilibrium valve closed, and allowing the engine to perform a voluntary retrograde movement, at the end of the working stroke, till it comes to rest by compressing the steam, the total weight of the pump-rods will be accurately denoted by the pressure of the column of water against the pumps, added to the pressure of steam upon the piston, minus the elasticity from imperfect vacuum beneath the piston.

3rd. The net pressure of the steam, last found, balances and measures the excess of the weight of pump-rods over the column of water. It counterpoises that portion of the entire weight which is requisite to overcome friction of all descriptions during the return stroke, and to deliver the water at the required velocity.

4th. The difference between the sums found by the 1st and 2nd propositions, is the value of engine and pit-work friction.

5th. The difference between the sums found by the 3rd and 4th propositions, is the joint value of the friction of the column of water, and of the weight necessary to displace it at the velocity of the return stroke.

The indicator, thus used as a pressure gauge, will unerringly furnish these important data, for the engine is at rest, and the steam quiescent when the observations are taken. The small space of time requisite to obtain them, cannot subject a mining engine to any inconvenience; and the absolute resistance being determined, together with the consumption of water as steam per stroke, the sufficiency, or insufficiency of the steam's mean simple elastic force throughout the stroke to overcome the resistance, may be readily determined on any engine.

Action of the cushion of steam.

The cushion, which is a quantity of steam recovered from the expiring stroke, and saved from annihilation in the condenser, is ingeniously used to break the shock in bringing the engine to a state of rest. It is also

whole stroke being unity. The second shews the economy of each engine in terms of the weight of water as steam consumed per stroke, compared with the expenditure of water as steam, had it been applied unexpansively, or expansively, but not percussively.—

Aliquot parts of the Stroke performed			
By the Steam's percussive action.	By Steam equal to the resistance.	By expansive Steam.	Engines.
Whole stroke unity.	Whole stroke unity.	Whole stroke unity.	
0·175	0·423	0·402	Huel Towan. (App. 12.)
0·251	0·296	0·453	Holmbush.
0·270	0·272	0·458	Fowey Consols.
Consumption of Water as Steam per Stroke.			
Actually consumed.	If used unexpansively throughout the stroke.	If used expansively, but not percussively	Engines.
lbs.	lbs.	lbs.	
6·642	15·720	8·051	Huel Towan. (App. 13.)
1·434	4·843	2·181	Holmbush.
3·737	13·717	5·116	Fowey Consols.

In the general table annexed, I have introduced many other quantities brought to light in the course of the investigation. To it I must refer the inquirer, having deemed it more perspicuous to confine the foregoing account of the analysis strictly to the argument, and to the production of such facts and quantities as were indispensable for the development of the theory advanced in explanation of the steam's action. For the same reason I have excluded from the text all the formulæ and computations. The processes used will be found in the Appendix, for one engine, the Huel Towan, which will serve to guide the inquirer in his search after the corresponding quantities for the two other examples, and also to inform those who may be disposed to pursue a similar method of investigating the action of steam in other engines.

greater evaporation would have been requisite, for much denser steam would have been required had not percussive action been brought into play, so as to aid, in the manner described, the steam's simple elastic force.*

I am now able to adduce the testimony of the diagram, which is a transcript of the steam's pressure upon the piston at every point of a stroke, that the steam's simple elastic force was insufficient to overcome the resistance opposed to it. The following are the pressures indicated at each 6 inches of the piston's descent in the cylinder:—

Inches of the Stroke.	Pressures. lbs. per square inch.	
6	27·0	
12	26·0	
18	24·4	
24	22·8	Steam's influx intercepted.
30	20·4	
36	17·4	
42	15·5	
48	14·2	
54	13·0	
60	12·1	
66	11·4	
72	10·6	
78	9·9	
84	9·4	
90	8·9	
96	8·4	
102	7·9	
108	7·6	
114	7·3	
120	7·0	
	20) 281·2	
	14·06 mean throughout the stroke.	

The steam's mean simple elastic force thus found is 14·06 lbs., and its force, as determined by the method of the volumes, 14·85 lbs. This difference

* Each pound of coal burnt must have evaporated 12¾ lbs. of water instead of 10½ lbs. to furnish steam of sufficient force to overcome the resistance.

Conclusion.

An accurate knowledge of the forces employed, and of their action, constitutes what is termed the theory of the engine, with which the practical engineer should be as intimately acquainted, as with its materials and mechanical structure. It is only by a clear perception of the former, that he can so arrange the latter, as to obtain the maximum effect from the power employed. The Cornish engineers have, unconsciously, applied the percussive action, as well as the elastic force of steam, and we have an instance in the Fowey Consols engine of $\frac{27}{100}$ths, or more than a quarter of the stroke being performed by percussive action alone; in other words, more than one-fourth of the effect was obtained without any cost of steam. If a method could be devised of withdrawing the steam again from the cylinder, after inflicting the blow upon the piston, these blows might be repeated by the same steam *ad infinitum;* but, since steam must necessarily accompany its percussive effort, the object of the constructor must be to appropriate the greatest possible quantity of this force, with the consumption of the least possible quantity of the steam's material ingredient.

Neither diagram marks the period of intercepting the steam. In fig. 8, there is no trace of it; in fig. 9 it would seem, from a superficial view, that the steam valve was closed where the upper horizontal line terminates, and expansion commenced. But such was not the fact, as that line of nearly equal pressure ceased at about 9 inches, *i. e.*, at about $\frac{1}{11}$th of the stroke, and the steam was admitted during about $\frac{1}{8}$th part.

The water load (11·4 lbs. per square inch on the piston) and other circumstances of this engine were so nearly similar to those of the Huel Towan, at the time of the experiments, that the deficiency of the steam's simple elastic force to overcome the resistance must at least have equalled the amount previously found for the Huel Towan engine.

The computation of these two diagrams verifies Mr. Henwood's observation that the temperature of a given volume of condensing water, when discharged, is inversely as the steam's elasticity in the boiler. It is manifest that a given volume of steam having an elasticity of 7·6 lbs. per square inch, contains less heat than the same volume at 8·5 lbs. per square inch. It justifies the notorious fact, and common belief in Cornwall, that more steam and fuel are consumed by the same engine as the pressure of steam falls in the boilers; and it confirms the deductions previously drawn from the varying temperature of the condensing water, (see page 273).

Similar facts are elicited from the computation of the diagrams of the Binner Downs engine (Henwood, figs. v. & vi.) In the first case the volume of steam in the cylinder, at the end of the working stroke, was 6·4 lbs., in the second case 7·4 lbs., being a difference of 1 lb. per square inch. The steam in these boilers varied in elasticity 16·78 lbs. per square inch, which thus produced an economy of about $\frac{1}{8}$th in favour of the higher steam, no change being made in the duration of the steam's admission.

APPENDIX.

Calculations for the Huel Towan Engine.

1. Required, the steam's elasticity in the cylinder at the end of the working stroke, from the volumes of steam and water consumed.

1st. Find the volume of water introduced into the cylinder per stroke.

Cub. ft.
847·5 total quantity evaporated from the temperature 93·8° (Henwood).
7·5 deducted for dilatation from 60°, and for impurities.

840·0 total consumption.

Cub. ft. Strokes. Cub. ft.
840 ÷ 7881 = 0·1065 water in steam entering the cylinder each stroke.

2nd. Find the volume of water existing in the cushion of steam, recovered.

lbs. per sq. in.
10·7 steam's elasticity in cushion (Indicator diagram).
Cub. ft.
9·176 volume of steam forming the cushion. (Henwood.)

The ratio which that volume of steam at 10·7 lbs. pressure bears to the volume of water from which it was generated, is 2365 —(*Vide* Table by M. de Pambour).

Ratio of steam
Cub. ft. steam. to water. Cub. ft.
Then, 9·176 ÷ 2365 = 0·0038 the constituent water in the cushion of steam at
lbs.
10·7 pressure.

Cub. ft. Cub. ft. Cub. ft.
And, 0·1065 + 0·0038 = 0·1103 total volume of water existing in the cylinder.

3rd. Find the volume of steam in the cylinder.

Contents of cushion. Described by the piston per stroke.
Cub. ft. Cub. ft. Cub. ft.
9·176 + 346·394 = 355·570 total volume of steam at the end of the stroke.

Steam. Water.
Cub. ft. Cub. ft.
And 355·570 ÷ 0·1103 = 3223 ratio of the volumes, denoting the steam's elasticity at the end of the working stroke to be 7·30 lbs. per square inch.

2. Required, the portion of the stroke performed by steam equal in pressure to the resistance.

lbs.
Resistance estimated at 18·01 per square inch against the engine piston.

1st. The entering steam has to raise the elasticity of the cushion from 10·7 lbs. to 18·01 lbs. which will require 0·0027 cubic feet of the water as steam introduced into the cylinder; found as follows. The ratio for the volumes at 18·01 lbs. is 1410; and 9·176 ÷ 1410 = 0·0065 which

Vol. of cushion. Vol. of water.
Cub. ft. Ratio. Cub. ft.

6. Required, the weight raised 1 foot by the elastic force of steam less than the resistance; that is, during its expansion below the pressure of the resistance.

Steam's elasticity, end of stroke.
lbs. lbs. lbs.
1st. 18·01 + 7·3 ÷ 2 = 12·55, the steam's mean force during expansion.

Area of piston.
Sq. in. lbs. ft. lbs.
2nd. 4988·08 × 12·55 × 5·775 = 361517 raised 1 foot.

7. Required, the total weight raised 1 foot by the steam's simple elastic force.

Raised 1 foot.
lbs.
379554 by steam not less than the resistance.
361517 by steam expanding below the resistance.

741071 total effect of the steam.

8. Required, the steam's mean pressure on the piston throughout the stroke.

Raised 1 ft. Length of stroke.
lbs. ft. lbs.
1st. 741071 ÷ 10 = 74107·1 mean load of steam on the piston.

Load on piston. Area of piston. Per sq. in.
lbs. Sq. in. lbs.
2nd. 74107·1 ÷ 4988·08 = 14·85 mean pressure of steam throughout the stroke.

9. Comparison between the resistance overcome, and the steam's simple elastic force.

Steam's mean force throughout stroke. Per sq. in.
Resistance.
lbs. per sq. in. lbs. per sq. in. lbs.
1st. 18·01 − 14·85 = 3·16 difference, being the steam's deficiency throughout the stroke.

Steam's mean force during expansion.
lbs. per sq. in.
2nd. 18·01 − 12·55 = 5·46 difference, being the steam's deficiency during its expansion.

Steam's force at the end of stroke.
lbs. per sq. in.
3rd. 18·01 − 7·30 = 10·71 difference, being the steam's deficiency at the end of the stroke.

lbs. lbs.
4th. As 18·01 : 1 :: 7·03 : 0·405, being the ratio between the absolute resistance and the steam's elasticity at the end of the working stroke.

10. Required, the portion of the stroke which the steam's simple elastic force was insufficient to perform.

1st. It has been found that during 5·775 feet (3), or 69·3 inches, the steam's elasticity was less than the resistance, and that, throughout that space, its mean force was only 12·55 lbs. per square

inch. The space, then, through which the expanding steam could have overcome a force equal to the absolute resistance will be inversely as those forces.

$$\therefore \overset{\text{lbs.}}{18{\cdot}01} : \overset{\text{Inches.}}{69{\cdot}3} :: \overset{\text{lbs.}}{12{\cdot}55} : \overset{\text{Inches.}}{48{\cdot}29}$$

and $\overset{\text{Inches.}}{69{\cdot}3} - \overset{\text{Inches.}}{48{\cdot}29} = \overset{\text{Inches.}}{21{\cdot}01}$ the portion of the stroke equivalent to the steam's deficiency of power.

2nd. The same is found by the relation which the steam's mean force throughout the whole stroke bore to the force requisite to counterpoise the resistance; thus,

$$\text{As } \overset{\substack{\text{Resistance.}\\ \text{lbs.}}}{18{\cdot}01} : \overset{\substack{\text{Total stroke.}\\ \text{Inches.}}}{120} :: \overset{\substack{\text{Steam's mean}\\ \text{force.}\\ \text{lbs.}}}{14{\cdot}86} : \overset{\text{Inches.}}{98{\cdot}94}$$

and $\overset{\text{Inches.}}{120} - \overset{\text{Inches.}}{98{\cdot}94} = \overset{\text{Inches.}}{21{\cdot}06}$ the steam's deficiency of power in terms of the stroke.

Obs. It has been found (2) that when the piston had passed through 50·7 inches, the steam's elasticity and the resistance were in equilibrio. Motion would then have ceased, but for the momentum transferred to the mass of pump-rods by the excess of the steam's force over 18·01 lbs. between the instant of its admission into the cylinder, and the instant when the piston had descended 50·7 inches. It was the discharge of this momentum which assisted the steam, during its expansion below the elasticity of 18·01 lbs., to drive the piston through the additional space of 48·29 inches; but it was unable to complete the whole stroke by 21·01 inches. The precise value of this momentum, or, what is the same thing, the value of the excess of force over the resistance during 50·7 inches of the stroke, is determinable as follows:—

The volume of steam in the cylinder when the piston had descended 98·99 inches would be 294·951 cubic feet, and its ratio to the volume of water 2770, denoting the elasticity to be 8·9 lbs. per square inch. The steam's mean force between 50·7 inches and 98·99 inches—that is, through 48·29 inches, the expansive stroke—would thus be $\overset{\text{lbs.}}{18{\cdot}01} + \overset{\text{lbs.}}{8{\cdot}9} \div 2 = 13{\cdot}45$ lbs. per square inch; and $\overset{\text{lbs.}}{18{\cdot}01} - \overset{\text{lbs.}}{13{\cdot}45} = \overset{\text{lbs.}}{4{\cdot}56}$ per square inch; being the value of the momentum derived from the steam's excess over the resistance during 50·7 inches, and discharged during the remaining 48·29 inches of the stroke which the steam was capable of performing.

Now, it appears by the Indicator diagram that the maximum elasticity of the steam was 27 lbs., and that its mean elasticity between the instant of the commencement of the piston's motion, and the instant of the steam's falling to 18 lbs., was about 22·50 lbs. per square inch; shewing that a quantity of momentum had been transferred to the mass, equal to the exertion of a force of about 4·50 lbs. per square inch, through 50·7 inches of the stroke, over and above the pressure of the resistance. More than this amount could not be restored during the steam's expansion below the pressure of the resistance, and it is accordingly found that the whole of it was discharged when the piston had attained about 99 inches of the stroke, for it is seen that the exertion of a force equal to 4·56 lbs. per square inch was required through 48·29 inches to enable the attenuating steam to carry the load through that space.

The correctness of the analytical process is thus confirmed by the evidence of the indicator diagram, and it is demonstrated that the engine would have come to rest at 99 inches of the actual stroke, but for the steam's initial percussive action, which transmitted to the mass a quantity of momentum

equivalent, with the additional expansion, to the exertion of 18 lbs. per square inch on the piston through 21 inches, and enabled it to complete the stroke.

The values of the momenta derived from these two distinct sources, viz., first, from the excess of the steam's simple elastic force over the resistance, during a portion of the stroke; and, secondly, from the steam's percussive action, are thus separable and determinable.—(*See* Diagram of the steam's action.)

11. Required, the absolute effect of 1 lb. of water as steam actually consumed, in pounds raised 1 foot.

1st. Find the weight of water consumed per stroke.

Total Water. Cub. ft.		Temp. 60°. lbs. per cub. ft.		No. of Strokes.		Water as steam. lbs.
840	×	62·3206	÷	7881	=	6·642 consumed per stroke.

Absolute effect. lbs. raised 1 ft.		lbs.		lbs.	
2nd. 898353·208	÷	6·642	=	135253	raised 1 foot by 1 lb. of water as steam.

12. Required, the aliquot parts of the stroke performed by the several forces as analytically investigated; the total length of stroke being unity.

	Stroke. Inches.			Inches.		Parts of stroke.	
As	120	: 1	::	21·01	:	0·175	performed by the steam's percussive action.
	120	: 1	::	50·70	:	0·423	performed by steam equal to the resistance.
	120	: 1	::	48·29	:	0·402	performed by the steam's expansive action.
				120·00		1·000	

13. Required, the consumption of water as steam, if used unexpansively throughout the stroke; and, if used expansively, but not percussively.

1st. It has been found that, if admitted gradually at the pressure of the resistance, the whole of the steam would have entered the cylinder when the piston had passed through 50·7 inches. To continue motion to the end of the stroke by the same gradual admission, the consumption of steam would be proportional to the length of stroke;

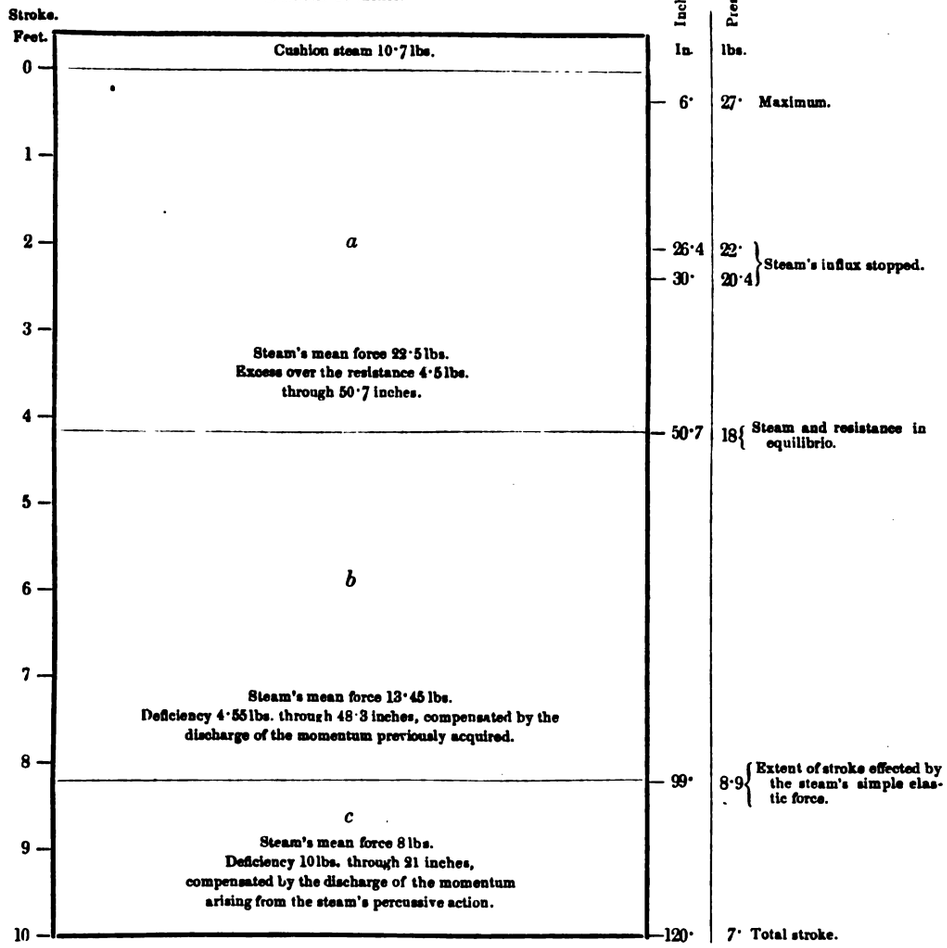
	Inches.		Water as steam. lbs.		Inches.		Water as steam. lbs.	
∴	50·7	:	6·642	::	120	:	15·720	consumption required had the steam been used unexpansively throughout the stroke.

2nd. It has been found that the steam's simple elastic force, unaided by percussive action, would have urged the piston through 98·99 inches;

	Inches.		Water as steam. lbs.		Inches.		Water as steam. lbs.	
∴	98·99	:	6·642	::	120	:	8·051	consumption required had the steam been used expansively, but not percussively.

HUEL TOWAN CYLINDER.

Diameter 80 inches.



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JUNE, 1840.

XI.—*On Setting-out Railway Curves.*

By CHARLES BOURNS, Assoc. Inst. C.E.

Read May 5th, 1840.

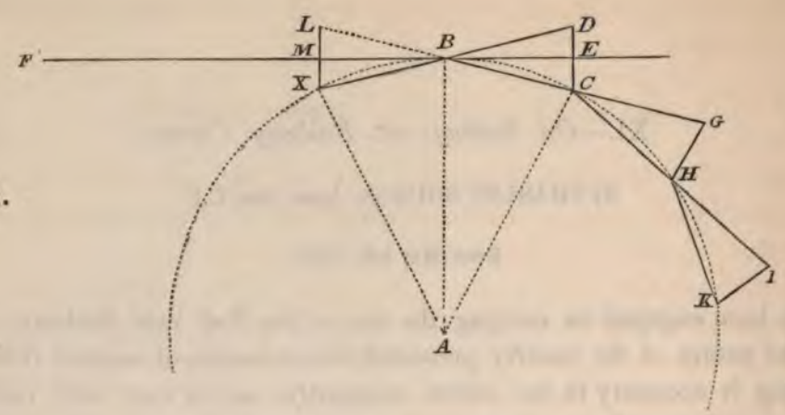
HAVING been engaged in ranging the line of the Taff Vale Railway, which from the nature of the country presented circumstances of unusual difficulty, rendering it necessary to use curves constantly, and to vary their radii and flexure very frequently, my attention was particularly drawn to the principles upon which the practice of setting-out a line was founded, so as to preserve a proper continuity of curvature.

Various methods have been hitherto proposed, none of which appeared to me to be generally practicable. This consideration, and the necessity for facilitating the labours of the field, induced me to investigate the subject; and, with the assistance of my friend Mr. Samuel Downing, I have been enabled to arrive at several results which are, I trust, not unworthy the attention of the profession.

It is not possible in practice to have a line of railway straight throughout, nor of one continuous degree of curvature; but it will consist of a system of curves and straight lines; the curves being not uncommonly of contrary flexure, and of different radii.

The cases of most frequent occurrence will be those of passing from a straight line into a curve, or from a curve into a straight line; but it is also sometimes necessary to pass from one curve to another of different radius, or through a point requiring a change of flexure. In all these cases the connexion must be tangential; that is, a straight line, or a curve, must be a tangent to the succeeding curve or straight line, at the point of junction. The ranging of lines on this principle may be made to depend on a construction of simple geometry; whence also may be determined the offsets which are to be measured. So that the whole practice of ranging a line will be performed with a chain, an offset-staff, and a few ranging poles. The following propositions will point out the manner in which this is to be effected.

Fig. 1.



PROPOSITION I.

"To set out a curve of given radius."

In the accompanying figure let A be the centre of the circle, a segment of which is to be set out; $F\,E$ a tangent at any point B; $B\,X$, $B\,C$ equal chords; let $B\,D$ be taken equal to $B\,X$, and join D and C, by the line cutting the tangent in E.

Then the triangles $B\,A\,X$, $B\,A\,C$, are equal and similar by construction; also the triangles $D\,B\,C$, $B\,A\,C$, are similar; for $B\,E$ being a tangent to the circle, and $B\,X$, $B\,C$, drawn cutting the circle from the point of contact, the angles $X\,B\,F$, $C\,B\,E$ will equal the angles in the alternate segments of the circle [Euclid, B. iii. Prop. 32]; that is, $D\,B\,E$ and $C\,B\,E$ equal respectively $\frac{1}{2}\,X\,A\,B$, and $\frac{1}{2}\,B\,A\,C$, the angles at the centre.

Therefore in the similar triangles $B\,A\,C$, $D\,B\,C$

$$A\,B : B\,C :: B\,C : D\,C;$$

that is, radius : chord :: chord : offset,

$$\text{or, offset} = \frac{(\text{chord})^2}{\text{radius}}$$

Hence if the common four-pole chain be used to set out a curve, the length of the offset at the extremity of every one chain chord produced will be known, and the point in the periphery marked, from the preceding simple construction.

Suppose the radius of the curve is 100 chains, and the chord one chain;

then reducing all to inches (the statute four-pole chain containing 792) we have, as above—

$$100 \times 792 : 792 :: 792 : \text{offset } (= 7 \cdot 92).$$

If any other standard of measure than the four-pole chain be used the radius and chord must be expressed accordingly. Hence whatever chain or standard of measure is used we have the following rule:—

"Divide the number of inches in that chain by the number of such chains in the radius of the curve, and the quotient will be the offset required in inches."

Practice.

Let it be required to set out a curve of 100 chains radius.

Stake out any tangent line, as $F\ B$; then the distance of the point C from this line produced to E (one chain), for a one-chain chord of the curve, will be $\frac{1}{2}\ D\ C$, or $3 \cdot 46$ inches; now one end of the chain being held at B, the position of the other end, C, will be ascertained by measuring $3 \cdot 46$ inches by means of an offset-staff, from the point E. That is, first measure one chain's length from B to E, and mark the point E, by a pole, or otherwise, and having one end of the chain held at B, move the other end from E, in the required direction, viz., towards C; the distance $E\ C$ being at the same time measured by an offset-staff, duly divided for the purpose.

As the angle $B\ E\ C$ is a right angle, an inconsiderable error is incurred by setting out an isosceles, instead of a right-angled triangle; but this would not be remedied in practice by setting out a right angle, because $B\ E$ is not truly a full chain in length.

The first point being determined, measure one chain on the chord $B\ C$ produced; that is, determine the point G, and if the end C is held fast, whilst the end G is moved to H, through the length of the full offset due to the required curve, measured with the staff as before, the point H is determined; and if $C\ H$ be produced to I, the point K may be determined in the same manner; and so on, by this simple use of the chain and offset-staff.

The offsets for curves of radii varying from 5 to 320 chains are given in a table at the end of this paper (p. 303).

Proposition II.

"To pass from a straight line into a curve of given radius."

This proposition is included in the preceding, supposing $F\,B$ to be the line from which it is required to pass into the curve $B\,C\,H$; the intermediate offset to the point C, and the successive points $H\,K$, &c. may be determined as already shown.

Proposition III.

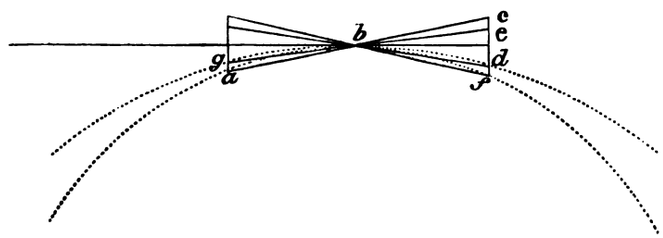
"To pass from a curve into a straight line."

Let $K\,C\,B$, Fig. 1, be the curve from which it is required to pass into the straight line $B\,F$. Produce $C\,B$ to L, that is, measure one chain forward; and as the distance $L\,M$ equals $C\,E$, let that distance, viz., half the offset of the given curve, be measured in the required direction, as before; when the line $B\,M$, being tangential to the curve, and in the desired direction, may be produced at pleasure.

Proposition IV.

"To pass from a curve of given radius to one of greater or less radius."

Fig. 2.



Case 1.

From a curve of given to one of less radius, as from the curve $g\,b\,d$ to the tangential curve $a\,b\,f$, of less radius.

Proceeding as before, the offset will be "the greater offset minus half the difference of the offsets." Thus, in Fig. 2, if the chord $g\,b$ is produced to e, the offset required will be $e\,f$; but $c\,f$ equals the greater offset, $e\,d$ the less, and $c\,e$ is half their difference.

Case 2.

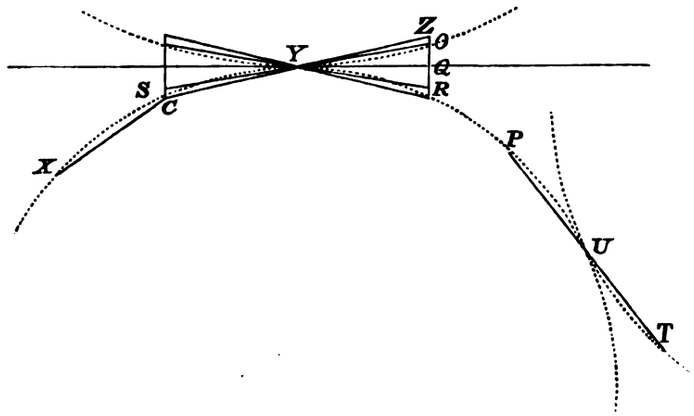
From a curve of given radius to one of greater radius, as from the curve $a\,b\,f$ (Fig. 2) to the tangential curve $g\,b\,d$, of greater radius.

The offset at the point of change will be "the less offset plus half the difference of the offsets." Thus, in Fig. 2, if the chord $a\,b$ is produced to c, the required offset will be $c\,d$, which equals $d\,e + e\,c$; but $d\,e$ equals the less offset, and $e\,c$ half the difference of the offsets.

PROPOSITION V.

"To pass from a curve of given radius and flexure into one of contrary flexure."

Fig. 3.



Case 1.

From a curve of given radius to one of greater.

In passing out of the curve $X\,Y$ into the tangential curve $Y\,O$, if the chord $C\,Y$ be produced to Z, so that $Y\,Z$ equals $C\,Y$, then the distance to be set off will be $Z\,O$; but as has been already shewn, $Z\,Q$ equals half the offset due to the curve $X\,Y$, and $O\,Q$ half that of the curve $Y\,O$; hence it is evident, that at the extremity of the first chain of the new curve it is necessary to set out "half the difference" (or perhaps more obviously, "the difference of the halves") "of the respective offsets due to the two curves."

When passing out of one curve into another of greater radius, as in the present case, this half difference must be set out "in the same direction as if proceeding with *the first curve*," as from Z to O.

Case 2.

From a curve of given radius to one of less radius.

When passing out of the curve $O\,Y$ (Fig. 3) into the curve $Y\,X$; if the chord $O\,Y$ be produced to S, so that $Y\,S$ equals $O\,Y$, the distance to be set off will be $S\,C$, which again is "half the difference of the offsets;" but in this case it is evident the half difference must be set out "on the same side as the succeeding offsets of the *new curve*."

Case 3.

From a curve of given radius into one of the same radius.

In this case, as there is no difference of offsets, so there is no distance to be set off, and "the two chords constitute one right line, as $P\,U\,T$ (Fig. 3). Of course this line must not be taken at either a greater or a less length than double the chord in use.

If the turn at Y or at U (Fig. 3.) be too short for the ground, a common tangent should be interposed between the curves.

PROPOSITION VI.

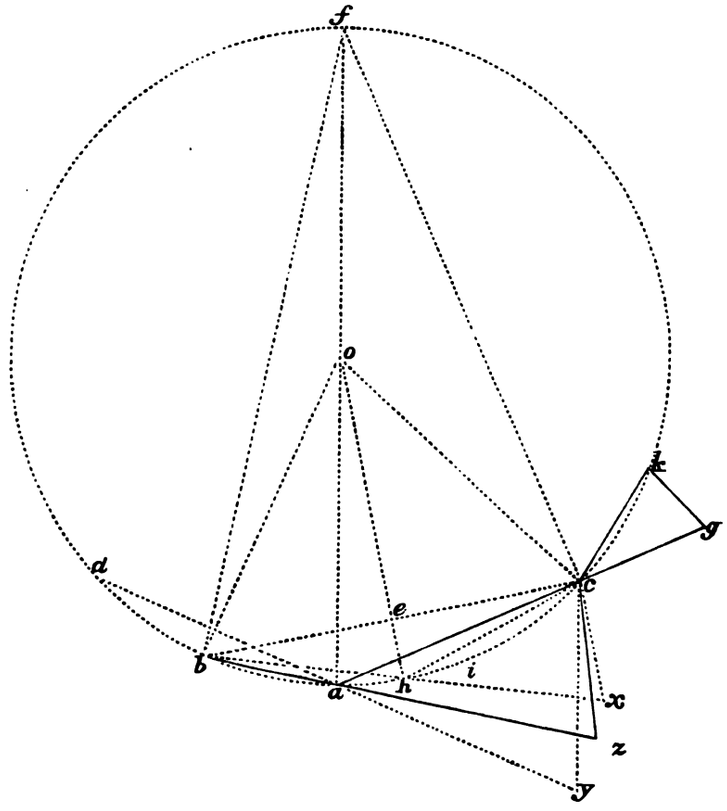
"To set out a curve when an obstruction occurs at a given point," as at i (Fig. 4).

In this case, as it is inconvenient at the point i to set out the usual chord, let any other chord be taken, as $a\,c$. Then $b\,a$ being the one-chain chord, let it be produced to z, and draw $a\,c = a\,z$; from the point a draw the diameter $a\,f$; draw $c\,f$ and $b\,f$, and the chord $b\,c$; bisect the arc $b\,c$ in h, and draw the radii $o\,b$, $o\,h$, and $o\,c$; draw $b\,h$ and $h\,c$; make $h\,x$ equal to $h\,c$; draw $a\,d$ equal to $a\,c$; connect the point c with x, z, and y, and those three lines, viz., $c\,x$, $c\,z$, and $c\,y$ will be in continued proportion; so that having found the first and third of them, the second being a mean proportional, will be known also.

Now $b\,f\,a$ and $a\,f\,c$ being right-angled triangles, $b\,f = \sqrt{a\,f^2 - a\,b^2}$, and $c\,f = \sqrt{a\,f^2 - a\,c^2}$; and $a\,b\,f\,c$ being a quadrilateral inscribed in a circle, the rectangle under the diagonals is equal to the sum of the rectangles under its opposite sides [Proposition CX., Cooley's Geometrical Propositions]; or $a\,f \times b\,c = a\,b \times c\,f + a\,c \times b\,f$; therefore $b\,c = \dfrac{a\,b \times c\,f + a\,c \times b\,f}{a\,f}$.

In the right-angled triangle $o\,e\,b$, $o\,b$ equals the radius, $b\,e$ equals half $b\,c$,

Fig. 4.



and $o\,e = \sqrt{o\,b^2 - b\,e^2}$; hence $o\,e$, and consequently $e\,h$, are known; and thus $c\,x$, which equals twice $e\,h$, is found.

The angles $b\,a\,c$ and $b\,h\,c$, being in the same arc of the circle, are equal; therefore their complements, $c\,a\,z$ and $c\,h\,x$, are equal; but those complements are respectively the vertical angles of isosceles triangles, therefore those triangles are similar, consequently $c\,x$ is to $c\,z$ as $c\,h$ is to $c\,a$; now $c\,x = \frac{c\,h^2}{o\,h}$, and $e\,y = \frac{c\,a^2}{o\,a}$ [Proposition I. of this paper]; that is, $c\,x : c\,y :: \frac{c\,h^2}{\text{rad.}} : \frac{c\,a^2}{\text{rad.}}$, consequently as $c\,h^2$ is to $c\,a^2$; but $c\,h^2 : c\,a^2 :: c\,x^2 : c\,z^2$; therefore $c\,x : c\,y :: c\,x^2 : c\,z^2$; then, multiplying the means and the extremes, $c\,x \times c\,z^2 = c\,y \times c\,x^2$, and dividing both sides by $c\,x$ we have $c\,z^2 = c\,y \times c\,x$, and therefore $c\,x : c\,z : c\,y$.

Practice.

Suppose ba one four-pole chain, and az two chains in length; and the radius 100 chains; then

$$bf = \sqrt{158400^2 - 792^2} = 158398 \cdot 02, \text{ and}$$
$$cf = \sqrt{158400^2 - 1584^2} = 158392 \cdot 08, \text{ and}$$
$$af \times bc = ab \times cf + ac \times bf = 376{,}348{,}991 \cdot 04; \text{ and}$$
$$bc = \frac{376{,}348{,}991 \cdot 04}{158400\ (= af)} = 2{,}375 \cdot 94; \text{ and } \frac{bc}{2} = be = 1187 \cdot 97.$$

Then $oe = \sqrt{ob^2 - be^2} = \sqrt{6{,}272{,}640{,}000 - 1{,}411{,}272 \cdot 7209}$
$= \sqrt{6{,}271{,}228{,}727 \cdot 280} = 79{,}191 \cdot 089$; and

$$eh = oh - oe = 79{,}200 - 79{,}191 \cdot 153 = 8 \cdot 911 = \frac{cx}{2}; \text{ therefore}$$

$cx = 17 \cdot 822$, and $cx^2 = 317 \cdot 624$; and cy (by Proposition I.) $= 31 \cdot 68$.

But $cx : cy :: cx^2 : cz^2$, therefore

$17 \cdot 822 : 31 \cdot 68 :: 317 \cdot 624 : 564 \cdot 600$; and the square root of $564 \cdot 600$ is 24, which is the value of cz, the offset required.

It will be found, by a similar computation, that the offset for the extremity of the next following one-chain chord, as at g, should be 12 inches, viz., half the length just ascertained.

If the chord, instead of being produced two chains' length, be taken half a chain in length, then the offset will be $2 \cdot 97$ inches; and for the next following full chain double as much, or $5 \cdot 94$ inches.

It will be perceived that the offset "for double the chord, is rather more than three times the offset for the one-chain chord;" and "that for the half chord, rather more than one-third of the usual offset." Hence it appears that, practically speaking, a good approximation only can be arrived at, if the usual length of chord be departed from. For permanent work, therefore, all obstructions should be removed, previously to setting-out the curves of a line of railway.

CHARLES BOURNS.

SEGMENTAL CURVES.

TABLE OF OFFSETS FOR EACH ONE-CHAIN CHORD PRODUCED.

Chains Radius.	Offsets.		Chains Radius.	Offsets.		Chains Radius.	Offsets.	
	ft.	in.		ft.	in.		ft.	in.
5 =	13	2·4	29 =	2	3·3	70 =	0	11·3
6 =	11	0·0	30 =	2	2·4	75 =	0	10·6
7 =	9	5·1	31 =	2	1·6	80 =	0	9·9
8 =	8	3·0	32 =	2	0·8	85 =	0	9·3
9 =	7	4·0	33 =	2	0·0	90 =	0	8·8
10 =	6	7·2	34 =	1	11·3	95 =	0	8·3
11 =	6	0·0	35 =	1	10·6	100 =	0	7·9
12 =	5	6·0	36 =	1	10·0	110 =	0	7·2
13 =	5	0·9	37 =	1	9·4	120 =	0	6·6
14 =	4	8·6	38 =	1	8·8	130 =	0	6·1
15 =	4	4·8	39 =	1	8·3	140 =	0	5·6
16 =	4	1·5	40 =	1	7·8	150 =	0	5·3
17 =	3	10·6	42 =	1	6·9	160 =	0	4·9
18 =	3	8·0	44 =	1	6·0	170 =	0	4·6
19 =	3	5·7	46 =	1	5·2	180 =	0	4·4
20 =	3	3·6	48 =	1	4·5	190 =	0	4·2
21 =	3	1·7	50 =	1	3·8	200 =	0	3·9
22 =	3	0·0	52 =	1	3·2	220 =	0	3·6
23 =	2	10·4	54 =	1	2·7	240 =	0	3·3
24 =	2	9·0	56 =	1	2·1	260 =	0	3·0
25 =	2	7·7	58 =	1	1·7	280 =	0	2·8
26 =	2	6·5	60 =	1	1·2	300 =	0	2·6
27 =	2	5·3	65 =	1	0·2	320 =	0	2·5
28 =	2	4·3						

XII.—*On the Locomotive Engines of the London and Birmingham Railway.*

By EDWARD BURY, M. Inst. C. E.

Read March 17th, 1840.

By the permission of the Board of Directors of the London and Birmingham Railway, I am enabled to furnish the Institution of Civil Engineers with the four Half-yearly Returns of the Locomotive department, on their line, between January 1839 and December 1840.

These returns are accompanied by a drawing of the Locomotive Engines to which they refer, with details of the principal parts; and as the quantity of coke consumed, as well as the cost of repairs, is much less than usual (which may, I conceive, be attributed to the system followed in the construction of the engines), I would make some observations upon those parts in which they more essentially differ from other locomotives. The first tabular statement of the performances of the engines is for the half-year ending 30th June 1839; it gives for each engine the number of miles it has passed over; the load conveyed, with the detailed cost of transport; the charges which cannot be fixed on any particular engine being proportioned among them. In the second half-year, ending the 31st December 1839, in addition to these particulars, is presented the distance performed by each engine from the time it first commenced working, together with the total repairs it has undergone during that period. It also shews the time it has been actually in motion during the periods named. The third and fourth returns are each preceded by a table, shewing the repairs the engines have undergone since the opening of the railway, as compared with the work they have performed. These accounts differ in form from the two preceding; they are much more detailed, and the engines of similar size and construction are classed together, so that the necessity of an account for each engine is avoided.

The London and Birmingham Railway is supplied in London with good coke made from Newcastle coal; but that obtained at Birmingham, from the Staffordshire coal, is of an inferior quality. For this reason the accounts

of the two kinds of coke are kept separate in the two first half-yearly returns.

During the whole of the year 1839 the average

	Quantity of Coke consumed per Mile run by the Engine.	Quantity of Coke consumed per Ton conveyed 1 Mile.	Cost of Repairs per Mile run by the Engine.	Cost of Repairs per Ton conveyed 1 Mile by the Engine.
For Passenger Engine	38·78 lbs.	0·86 lbs.	2·51 pence.	0·55 pence.
For Merchandize Engine	42·58 lbs.	0·56 lbs.	2·1 pence.	0·28 pence.

When compared with similar results obtained on other railroads, these returns exhibit a great economy in both items, which I attribute chiefly to the shape of the fire-box, to the inside framing, and to the use of four wheels instead of six.

The first engine which I made upon this construction was the "Liverpool;" it was commenced in October 1829, and set to work on the Liverpool and Manchester Railway in July 1830; since that period many improvements in the details have been introduced, but the general plan of construction has been steadily adhered to.

The Boiler. The most essential requisites in a locomotive boiler are, a great extent of heating surface in a small compass, and the arrangement of that surface so as to cause and promote a rapid circulation of the water. These principles have been kept in view in the disposition of the tubes and the shape of the fire-box.

Circulation of Water. The central tubes and the centre of the fire-box are nearest to the surface of the water, which is consequently hottest and lightest above those parts, and towards them the particles of water or globules of steam will rush; the outer tubes and sides of the fire-box being lower, and consequently cooler, establish a return current of the colder particles of water from the centre towards the sides and bottom of the boiler.

Fire-Tubes. This arrangement of the upper row of tubes has also the advantage of preventing any of them from being uncovered when the engine is travelling on sharp curves, where the centrifugal force throws the water to one side of the boiler.

Fire-Box. The fire-box is cylindrical, with its back flattened to receive the ends of the tubes: its top is a sphere merging into a cylinder or elongated cone, and all the curves are such as to enable the plates to resist the pressure of the steam without the assistance of ribs or stays, which so materially prevent the circulation of the water over square fire-boxes. The cylindrical-shaped fire-

box possesses a great superiority over a square one, inasmuch as the corners, in which the combustion is always languid, are avoided.

Lead-plug in the Fire-box. A lead-plug is placed at the culminant point of the dome-shaped top, and will therefore melt before any other part of the fire-box is left dry: in a fire-box with a flat top this would only occur when the whole was dry and probably injured.

The fire-box is made of wrought iron three-eighths of an inch thick, except the tube plate, which is half an inch thick. The joints are welded wherever they are in contact with the burning fuel, as a rivetted joint, from its presenting a double thickness of metal, will not long resist the intense heat to which it is exposed. If it were made of copper instead of iron the thickness of the metal must be greater, and the weight would be increased; and it will be seen, in a comparison of the engines having four and six wheels, that the lightness of the iron fire-box is a point of considerable importance.

Consumption of Water and Coke. It is practically found that the passenger engine consumes 75, and the merchandize engine 85 cubic feet of water per hour. This quantity is greater than that calculated from the steam's elasticity in the cylinders, and from the number of cylinders filled, and emptied, at the respective mean velocity of 30 miles per hour for the passenger, and 25 miles per hour for the merchandize engine. The difference is owing to the escape of steam through the safety-valves, and to the occasional slipping of the wheels on the rails when the load is heavy.

Hence the superficial heating surface to evaporate one cubic foot of water in an hour, will be in the boiler of the passenger engine 5·6 square feet, and in the boiler of the merchandize engine 6· square feet of heated surface. This is nearly double the effect produced in stationary boilers, and yet the result is obtained without any considerable sacrifice of fuel; for, including all the coke which is wasted throughout the year, when the fires are drawn or lighted, and that which is used in the workshops, &c. the merchandize engines only consume 12 lbs., and the passenger engines 15 lbs. of coke per cubic foot of water evaporated.

Taken together, these results are superior to any thing that has been obtained from long trials of any other description of locomotive boiler.

Steam Regulator. The other parts connected with the boiler which differ from the usual mode of construction are the steam cock, or regulator. This is shewn in detail in the Figures 1, 2, and 3, drawn to a scale of three inches to one foot.

Figure 1 gives a view of the plug or valve, which, when closed, fits into the seat *e* of Figure 3. It is provided with a handle (shewn broken at *f*)by which it can be turned round a quarter of a circle; a groove, or screw thread, of a four-inch pitch, is cut upon it at *a*, and when in its place the stud *b* is fitted into it. By this means, when the plug is made to revolve a quarter of a circle, the face *e* is drawn back one inch from its seat, leaving eleven square inches of passage for the steam, which flows in the direction of the curved arrow, without the passage being at all throttled or obstructed. When it is necessary to throttle the steam, the handle is fixed at any required angle between the points marked "open" and "shut," so that the steam-way is then accurately in proportion with that angle.

Figure 2 represents the mode of fixing the stud *b* in the groove *a*.

Fig. 1.

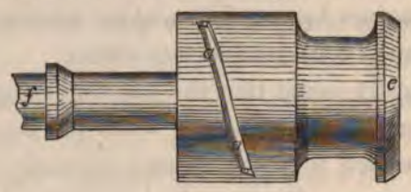
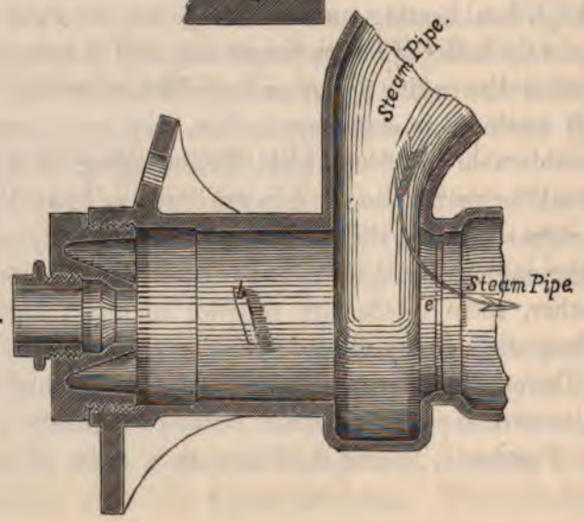


Fig. 2.

Fig. 3.



When the valve *e* is in its "seating," and the handle turned to "shut" in the quadrant, the stud *b* is dropped through a hole, which it fits accurately, as well as into the groove *a*, which is seen opposite the hole; at the back of the stud *b* are placed a strip of canvas and one of tin, and over them is driven a dove-tailed wedge *c*, which pressing on the elastic canvas renders the whole steam-tight.

This regulator enables the engine driver to govern the supply of steam with great precision, and it is not liable to get out of repair.

Force Pump. The section of the coupled engine (Plate I.) shews a longitudinal section through the pump, and Figures 4 and 5 (p. 310) are two transverse sections of it.

Figure 4, *h* is the working barrel, *i* is an intermediate pipe connecting it to the valve-box, and *g* is the delivery pipe fixed to the side of the boiler.

k is the inside frame, to which the pump is firmly secured; all the valves are the ordinary "clack-valves," *d* being an additional one to prevent any possibility of the return of hot water into the pump. The rod *l* is used to force the valve *a* into its seat, should the valve *b* not work perfectly tight: the small cock *f* is placed to ascertain if the flow of water into the pump is perfect.

Figure 5 is a section through the suction-valve, the play of the valve being limited by the piece *e*.

The pump is so constructed that any of the valves can be examined or replaced in a few minutes, and one of the pumps is more than sufficient for the supply of the boiler.

The Framing. The detailed drawing of the frame of the 13-inch coupled engine (Plate II.) shews the manner in which it connects all the parts of the engine and boiler together. (*See* the description of Plate II., p. 317.)

Next to a good boiler, which governs the economy of fuel, the most important point in the construction of a locomotive (inasmuch as it most materially influences the cost of repair) is to connect all the parts firmly together by a strong and well-arranged framing, so that they shall retain their relative positions when the engine is in motion, and that it shall receive and bear the strain and the concussions to which every part is subject. The inside framing possesses a great superiority, in this respect, over the outside framing, as it

Fig. 4.

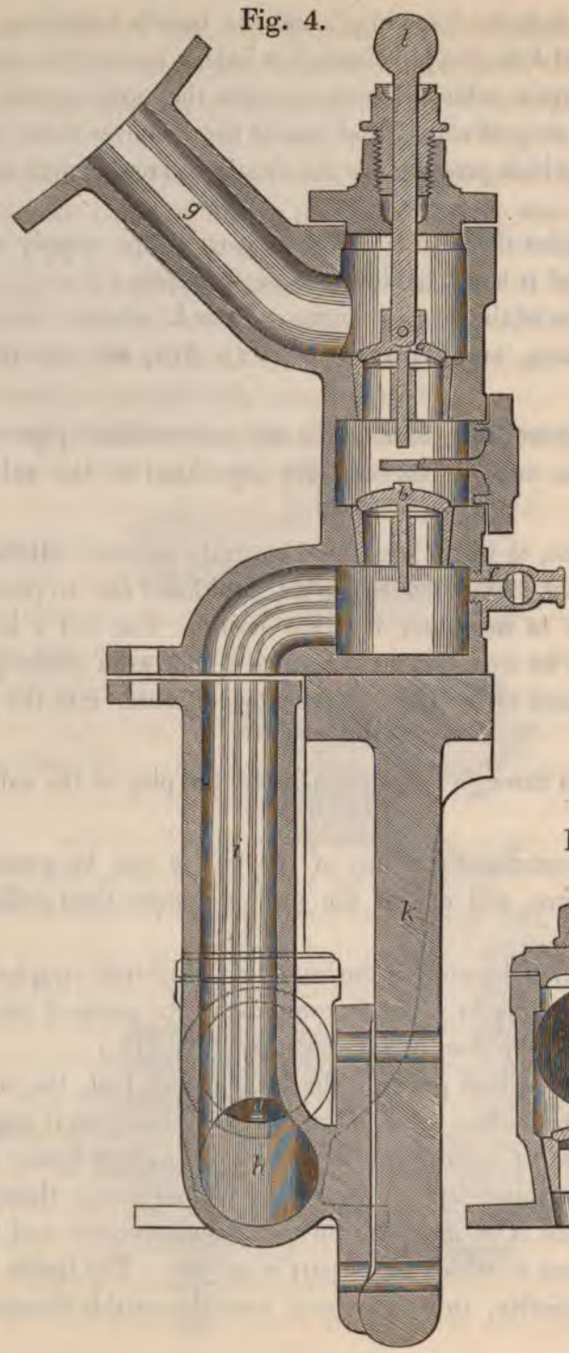
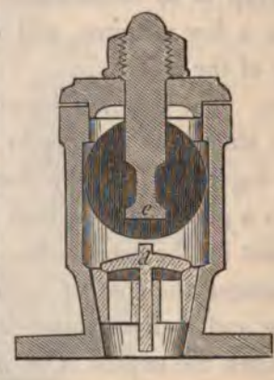


Fig. 5.



forms a stronger and more direct connexion between the cylinder, the cranked axle, and all the moving parts; and it bears all the strain of the engine without throwing any portion of it on the boiler, as is the case with the outside framing.

Comparison of inside with outside Framing.

These advantages are best described by comparing it with the ordinary outside framing when submitted to the principal strains which it has to resist.

The most important is that caused by the whole power of the engine acting as a direct strain upon the crank as it passes over either centre.

With the inside framing the centre line of the connecting rod is only ten inches distant from the centre line of the frame, and the total distance between the bearings is 43½ inches; but where the framing is outside the wheels, these dimensions are necessarily 20 inches and 72 inches respectively, and the effect of the strain on the crank in this case would be to its effect with the inside framing as 14 is to 8.

For this reason, when the principal frame is placed outside the wheels, it becomes necessary to have an additional inside framing, to prevent the fracture of the axle; these additional inside frames not only cause an increase of friction on the bearings of the cranked axle, but also throw a considerable strain on the boiler, which then becomes the medium of connexion between the inside and outside frames, the inside frames being fixed at one end to the bottom of the smoke-box, and at the other end to the fire-box; while the principal frame is attached by long brackets to the body of the boiler.

The fact that the use of four additional inside frames occasions six bearings on the axle (that axle being only six feet long), renders the system of principal outside framings so objectionable, that that circumstance alone should suffice to cause their rejection; for it is well known to practical men, that it is impossible to key so many bearings perfectly true, and to maintain them so, when the engine is working; and even if this precision were attained, the aggregate friction on the four inside and the two outside bearings would be much greater than when it is all thrown upon two bearings; because in the first place all the friction due to the weight of the boiler is borne by the two outside bearings alone, and that which results from the pressure of the steam, through the medium of the connecting rod, is thrown upon the four inside bearings; the pressure on the outside bearings is vertical, and the mean pressure on the inside bearings is nearly horizontal. So that if, instead of acting

separately, these two amounts of pressure were thrown on the same bearings, the friction would only be due to the resultant of the pressures, and would consequently be much reduced.

Friction on two Bearings. The friction on the cranked axle, having only two bearings, as where a single inside frame is used, will be, under ordinary circumstances, due to the resultant of the vertical and horizontal pressures, or—

$$\frac{\sqrt{12646^2 + 3000^2}}{8} = \frac{13000 \text{ lbs.}}{8} = 1625 \text{ lbs.}$$

In addition to the friction resulting from these forces there is a considerable pressure on the bearings, arising from the tightness of the brasses; and it is evident that the friction arising from this cause will be three times greater with six than with two bearings.

Strain on the Axles. Another important feature is the strain to which locomotive engines are liable, from the pressing or striking of the flanges of the wheels against the rail when travelling on a curve. In engines with the bearings inside the wheels, the weight of the boiler has a tendency to bend the axle down in the centre; while the pressure of the flange against the rail acts upon it in a contrary direction, and thus one strain counteracts the effect of the other. If the bearing is outside the wheel, the weight of the boiler tends to bend the axle upwards, and a strain on the flange of the wheel acts in the same direction and in addition to it.

The position of the bearings inside the wheels is of great practical advantage in case of the fracture of the cranked axle, as the weight on the bearings presses the flange of the wheel against the rail and assists the length of the journal in keeping it from being thrown off the rails. Instances have occurred on the London and Birmingham Railway, when an axle has broken, that not only have the wheels remained on the rails, but the driver has been enabled to proceed with the train to the nearest station.

Stiffness of the Framing. The stiffness of the single inside framing is not only a remedy against the excessive wear and tear which is consequent on a less perfect union between the parts of the engine, but its simplicity allows the whole machinery to be arranged in a more compact form and constructed with greater solidity. The valve gear is much simpler in its construction, and the engine driver, while standing on the foot-plate, can inspect the whole of the machine, and detect any derangement requiring his attention.

The Excentrics. The four excentrics are placed side by side, and are firmly connected together by bolts and snugs, so that the angle at which each is fixed cannot vary. This angle is governed in some measure by the nature of the work the engine is destined to perform. On the London and Birmingham Railway the excentrics are generally so placed as to cut off the steam when the piston is within $2\frac{3}{4}$ inches from the end of the stroke.

On the relative Advantages of Four and Six Wheels.

Comparison of four and six-wheeled Engines. It is admitted that a locomotive engine should be as light as is consistent with great strength, simple in its construction, be composed of as few parts as possible, and that the greatest regard is to be had to the diminution of friction; such being the case, it is obvious that four wheels must be preferable to six, provided that they carry the engine equally well.

The use of six wheels originated in the necessity of supporting the large and heavy fire-box of the engine, which was not sufficiently balanced by the smoke-box end; no such necessity exists in the locomotives of the London and Birmingham Railway, as the weight is nearly equally distributed on the front and hind wheels, and not only would two additional wheels be useless, but they would be prejudicial, especially when the engines are travelling upon curves.

A four-wheeled engine, travelling upon a curve, is driven, by the direct application of the moving power, towards the outside of the curve; but as the wheels are rather conical, the larger diameter of the cone will ride on the outside rail, while the smaller diameter of the opposite wheel will bear on the inside rail, and this difference (as the outside rail is longer than the inside one) will allow both the wheels to revolve without slipping or grinding.

With an engine upon six wheels, if the two leading wheels assumed this position, the others would necessarily be dragged after them; but a still more important case is, that the angle which the centre line of the locomotive forms with the tangent of the curve in which it is caused to move, is much greater with six wheels than with four; so that the flange of the wheel presses more against the rail with the former than with the latter engine.

The pressure against the outside rail arising from this cause, will be in direct proportion to the distance between the front and hind axle of either engine, so that it will be nearly as 10 : 6. This pressure and consequent

friction is still further increased by the action of the middle wheel, which tends to ride on the same curve as the front and hind wheels; but is prevented from doing so by being in a straight line between the two, and is thus forced to move laterally between the chord and the circumference of the curve. The friction arising from this lateral motion further presses the engine against the outside rail. Thus the four-wheeled locomotive has in proportion a greater weight on the front wheels, it presses less against the outside rail, and offers much less friction when travelling on curves—hence it has less tendency to be thrown off the rails; it is simple in its construction, less expensive in repairs on account of this simplicity, and its durability, as shewn in the accompanying Tables of the duty performed, and the small cost of it, fully justifies the preference given by the Directors of the London and Birmingham Railway to this description of engine.

EDWARD BURY.

Liverpool, 1840.

DESCRIPTION OF PLATE I.

The Plate represents the Merchandize Engine, with cylinders 13 inches diameter, and coupled wheels.

Fig. 1. Is a sectional elevation.

Fig. 2. A sectional plan, the line *x x* shewing the principal line of section followed in Fig. 1.

Fig. 3. A transverse section through the smoke-box.

To render the drawing more complete, several parts are shewn in each figure which do not come into view on the line of section.

The letters of reference correspond in all the figures.

A The fire-box.

B The fire-tubes; there are 96 tubes, 2 inches diameter each, and 9 feet long.

C The smoke-box.

D The regulator.

E The steam pipe, $3\frac{1}{2}$ inches diameter.

F The safety-valve and spring pressure gauge, $2\frac{1}{2}$ inches diameter.

G The locked-up safety-valve, $2\frac{1}{2}$ inches diameter.

H The damper.

I The buffer bar.

J The steam whistle.

L The steam cylinders, 13 inches diameter, 18 inches stroke.

M The force-pumps, plunger 2 inches diameter, 18 inches stroke.

N The cranked axle; the journals are 5 inches diameter and 7 inches long, the bearing of each crank is $5\frac{1}{2}$ inches diameter and $3\frac{1}{2}$ inches long.

O The connecting rods, oval-shaped, 2 inches by $2\frac{1}{2}$.

P The axle of the front wheels, $4\frac{1}{2}$ inches diameter.

Q The springs.—The springs for the cranked axle are composed of 16 plates, together $4\frac{1}{8}$ inches deep at the centre; those for the front axle are composed of 10 plates, together $3\frac{7}{8}$ inches deep at the centre.

a a The steam pistons, of gun metal. The packing consists of 2 rings of cast-iron segments forced outwards by brass wedges and steel springs. The piston-rods are 2 inches diameter.

b The inlet passages for the steam, $1\frac{1}{4} \times 6\frac{1}{2}$ inches.

c The outlet passage for the steam $1\frac{7}{8} \times 6\frac{1}{2}$ inches.

d The slide-valves.

d' The slide-valve rods, 1 inch diameter.

e The pendulum rods, for carrying the ends of the excentrics.

f The shaft, to which the excentric levers are fixed.

g The shaft connecting the motion of the lever *h*, and the rod *i*, to the shaft.
h The guides for the piston-rods.
i Steadying pieces for the guides.
j Shaft carrying the steadying pieces.
k l The rods for moving the slide-valves.
k′ l′ The levers of the hand-gear.
m The shaft carrying the valve trappings.
n The lever for working the valves.
n′ The lever worked by the excentrics.
p The excentrics for the retrograde motion.
q The excentrics for the advancing motion.
r The pipes (2 inches diameter) connecting the force-pumps with the tenders.
s The cock for letting the water out of the boiler.
t The rods ($1\frac{1}{8}$ inch diameter) for coupling together the front wheels and the driving wheels.
u A lead plug at the top of the fire-box.

DESCRIPTION OF PLATE II.

This plate shews the details of the framing of the Merchandize Engine. The letters correspond in all the figures.

a a Two straps firmly rivetted on each side to the frame, passing beneath and half round the cylinders, which are connected to them by being accurately fitted into snugs cast on the cylinders and rivetted to them.

b The crank axle.

c The axle of the front wheels.

d The springs.

e The transverse shaft for the steadying pieces of the guides.

f The bolt-holes for fixing the force-pumps on the frame.

g The buffer bar.

h The bolts for fixing the buffer bar to the frame.

i The bolt holes for the suspension rods of the springs.

k The cotter joints, connecting the lower with the upper part of the frame.

l & *m* Holes for receiving the draw-pin for connecting the tender with the engine.

n The holes for fixing the plate to the frame.

p The bracket connecting the frame with the fire-box.

q " " " body of the boiler.

r The strap connecting the lower part of the frame with the bottom of the fire-box, into which it is dovetailed and bolted.

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITURE,
From 1st January to 30th June 1839.

PASSENGER ENGINES.

Number of Engine.	Miles Run.	Tons conveyed One Mile, exclusive of the Weight of the Engine and Tender.	COKE.						OIL.						Hose Pipes.		
			London (Good).	Birmingham (Inferior).	Total	Cost.			Quarts.	Cost.							
			Cwts.	Cwts.	Cwts	£.	s.	d.		£.	s.	d.			£.	s.	d.
1	7874	406,910	2150	144	2294	224	10	0	201	8	7	6			1	13	9
2	7294	330,568	2252	216	2468	238	14	0	162	6	15	0			0	10	6
3	7986	379,614	2448	241	2689	258	17	3	228½	9	10	5			2	0	0
4	7769	449,124	2664	114	2778	273	10	6	208½	8	13	9			..		
5	8987	333,782	1838	820	2658	235	13	1	217½	9	1	3			0	10	6
6	8661	365,550	2114	724	2838	257	4	6	246	10	5	0			2	9	6
7	14822	650,246	3699	798	4497	420	7	0	400½	16	13	9			..		
8	11548	452,254	2293	1040	3333	294	6	0	291	12	2	6			..		
9	10113	451,220	2572	698	3270	300	16	6	245½	10	4	7			..		
10	5656	276,108	1821	199	2020	194	10	9	157½	6	11	3			1	4	3
11	4672	201,036	1529	415	1944	179	3	0	165½	6	17	11			..		
12	5546	231,192	1616	785	2401	210	13	3	209	8	14	2			0	10	6
13	9044	358,190	2172	1038	3210	282	14	2	249½	10	7	11			0	5	3
14	4804	214,010	1308	244	1552	147	11	9	128	5	6	8			..		
15	8468	418,994	2479	153	2632	257	9	3	182½	7	12	1			0	9	6
16	5053	240,012	1657	212	1869	178	19	0	154½	6	8	9			..		
17	6822	287,552	1888	253	2141	204	12	3	148½	6	3	9			..		
18	5226	215,040	1497	327	1824	170	2	9	121	5	0	10			1	1	9
19	6185	210,110	1459	704	2163	190	0	8	166½	6	18	9			..		
20	8297	333,878	2312	283	2595	248	17	9	182½	7	12	1			0	5	3
21	4689	222,728	1564	175	1739	167	6	9	135½	5	12	11			..		
22	4666	192,272	1317	357	1674	154	0	3	138½	5	15	5			0	9	6
23	4920	226,882	1593	149	1742	168	12	3	149½	6	4	7			0	10	6
24	7688	378,062	2313	122	2435	238	18	6	183½	7	12	11			0	9	6
25	4375	224,788	1561	162	1723	166	4	6	114½	4	15	5			..		
26	2275	89,338	811	308	1119	100	9	0	79½	3	6	3			1	13	0
27	7330	270,182	1635	833	2468	215	11	3	178½	7	8	9			2	14	9
28	12160	463,098	2796	1341	4137	363	13	8	222	9	5	0			0	15	9
29	5986	240,506	1566	664	2230	198	2	0	181	7	10	10			0	10	6
30	7606	236,982	1993	262	2255	215	13	6	173	7	4	2			0	14	9
31	5132	236,588	1513	161	1674	161	7	3	138	5	15	0			..		
32	6287	296,788	2027	196	2223	214	19	0	178	7	8	4			0	9	6
33	5026	215,680	1220	604	1824	159	15	0	128	5	6	8			0	16	6
34	6641	254,542	1515	819	2334	202	13	9	220½	9	3	9			..		
35	4682	234,922	1156	575	1731	151	10	9	132	5	10	0			0	16	6
36	8822	376,896	2029	876	2905	257	13	0	258	10	15	0			6	10	3
	253,112	10,965,644	68,377	17,012	85,389	7905	3	10	6675½	278	2	11			27	11	9

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITURE, from 1st January to 30th June 1839, inclusive—(*continued*).

PASSENGER ENGINES.

Number of Engine.	Fire Tools.	Enginemen's and Firemen's Wages.	REPAIRS. Engine.	REPAIRS. Tender.	Files.	Summary.	Proportion of General Charges.
	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
1	0 4 8	45 4 8	74 4 5·50	14 0 5·8	0 11 3	368 16 8·58	114 4 6·47
2	..	49 3 4	29 7 0·29	7 7 0·28	0 4 5	332 1 3·57	103 5 9·43
3	0 11 1	45 11 9	38 5 3·67	5 3 8·71	0 12 9½	360 12 3·88	112 3 4·86
4	0 9 4	49 1 3	24 10 6·28	18 11 2·25	0 17 8	375 14 2·53	116 17 3·69
5	..	58 10 0	34 16 6·48	10 7 2·31	1 6 8½	350 5 3·29	108 19 0·05
6	..	59 2 6	66 0 4·36	2 6 7·32	0 15 0½	398 3 6·18	123 17 0·77
7	0 4 0	77 0 5	103 7 6·12	8 7 10·62	1 0 7	627 1 1·74	195 0 11·32
8	..	69 6 8	29 15 8·24	17 17 4·75	1 13 10	425 2 0·99	132 4 7·04
9	0 4 0	66 11 4	34 5 8·57	11 4 10·25	0 11 5	423 18 4·82	131 17 2·65
10	..	39 3 5½	80 16 7·20	8 3 5·50	2 9 9	332 19 6·20	103 11 5·44
11	0 4 8	63 14 0	51 3 9·11	9 16 10·28	1 12 2	312 12 4·39	97 4 9·70
12	..	56 15 4	38 16 2·35	14 1 5·79	1 13 7½	331 4 6·64	103 0 6·94
13	0 8 0	64 14 4	41 0 4·65	3 5 7·78	0 9 4	403 5 0·43	125 8 5·71
14	0 0 6½	30 18 1	15 4 7·65	1 7 10	0 12 8	201 2 2·15	62 11 1·26
15	..	51 0 11	41 5 11·41	0 1 3	1 5 6¾	359 4 6·16	111 14 9·05
16	0 4 8	40 8 1½	42 15 9·47	2 8 1·11	1 13 6	272 17 11·08	84 17 8·35
17	0 0 6	42 14 4	48 5 6·92	12 3 11·90	1 9 9½	315 10 2·32	98 2 9·55
18	0 5 8	44 4 6	75 2 11·10	10 1 4·97	1 0 7¼	307 0 5·32	95 9 11·94
19	..	42 9 1	92 6 7·48	3 11 9	1 4 1	336 10 11·48	104 13 8·10
20	0 5 6	47 5 6	55 17 9·87	4 17 2·38	0 19 11	366 1 0·25	113 17 2·58
21	0 4 8	31 10 6	25 1 6·81	11 4 5·75	0 17 5	241 18 3·56	75 4 11·52
22	0 4 8	44 5 2	32 5 1·70	1 10 2·87	0 14 2½	239 4 7·07	74 8 3·05
23	..	45 5 7	55 2 5·24	8 12 3·90	1 8 11	285 16 7·14	88 18 1·87
24	0 1 8	46 2 3	79 14 0·56	12 9 6·50	1 16 9	387 5 2·06	120 9 1·78
25	0 4 8	37 12 8	50 2 5·51	12 3 4·12	0 18 4	272 1 4·63	84 12 6·63
26	..	33 9 2	12 19 3·70	7 17 11·50	0 2 4	159 17 0·20	49 14 5·24
27	0 0 10	54 5 0	57 14 4·95	3 3 9·75	0 17 5½	341 16 3·20	106 6 5·18
28	0 4 0	73 9 7	81 0 11·29	9 3 8·43	1 15 11½	539 8 7·22	167 15 9·75
29	0 8 8	44 5 11	54 4 5·4	1 5 0·50	0 12 3	306 19 7·54	95 9 8·89
30	0 2 6½	45 0 1	52 8 11·21	3 5 9·15	1 8 11½	325 18 8·36	101 7 7·78
31	..	34 1 0	118 0 5·35	12 9 11·90	3 8 11	335 2 7·25	104 4 10·27
32	0 4 8	40 4 2	63 2 10·45	1 4 11·43	2 2 1½	329 15 7·38	102 11 6·92
33	0 4 0	44 9 6	70 15 11·67	11 14 2·50	0 15 3	293 17 1·17	91 8 0·99
34	..	66 13 4	42 14 6·3	5 9 4·9	2 5 0	328 19 8·12	102 6 7·46
35	0 4 8	41 7 6	70 2 3·32	2 16 3	3 0 10	275 8 9·32	85 13 6·16
36	0 8 8	61 15 0	83 15 3·51	2 4 9·75	3 8 4	426 10 4·26	132 13 4·57
	5 16 4	1786 16 0	1966 14 5·06	272 1 0·42	47 17 9	12,290 4 0·48	3822 17 6·96

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITURE, from 1st January to 30th June 1839, inclusive—(*continued*).

PASSENGER ENGINES.

Number of Engine.	Total Cost.			Average of Coke. Weight. Per Mile Run.	Average of Coke. Weight. Per Ton per Mile.	Average of Coke. Cost. Per Mile Run.	Average of Coke. Cost. Per Ton per Mile.	Average of Oil. Per Mile Run.	Average of Oil. Per Ton per Mile.	Cost of Repairs. Per Mile Run.	Cost of Repairs. Per Ton per Mile.	Total Cost. Per Mile Run.	Total Cost. Per Ton per Mile.
	£.	s.	d.	lbs.	lbs.	d.	d.	Pints.	Pints.	d.	d.	d.	d.
1	483	11	3·05	32·63	·631	6·84	·132	·051	·98	2·69	·052	14·74	·285
2	435	7	1	37·89	·836	7·43	·163	·044	·98	1·20	·026	14·32	·316
3	472	15	8·74	37·71	·793	7·78	·138	·056	·120	1·30	·027	14·20	·298
4	492	11	6·22	41·33	·692	8·45	·146	·053	·92	1·33	·023	15·21	·263
5	459	4	3·34	33·12	·891	6·29	·169	·048	·130	1·20	·032	12·26	·330
6	522	0	6·95	36·69	·869	7·12	·168	·056	·134	1·89	·044	14·46	·343
7	822	2	1·06	34·	·774	6·80	·155	·054	·123	1·88	·041	13·30	·303
8	557	6	8·03	32·32	·825	6·11	·156	·050	·128	·99	·025	11·58	·295
9	555	15	7·47	36·21	·811	7·13	·160	·048	·108	1·08	·024	13·18	·295
10	436	10	11·64	40·	·819	8·24	·169	·056	·114	3·77	·077	18·52	·379
11	409	17	2·09	46·60	1·083	9·20	·213	·070	·164	3·13	·072	21·05	·489
12	434	5	1·58	48·30	1·163	9·11	·218	·075	·180	2·28	·054	18·79	·407
13	528	13	6·14	39·75	1·003	7·50	·189	·055	·139	1·17	·029	14·03	·354
14	263	13	3·41	36·18	·812	7·37	·165	·053	·119	·83	·018	13·17	·295
15	470	19	3·21	34·81	·703	7·29	·147	·043	·89	1·17	·023	13·34	·270
16	357	15	7·43	41·42	·872	8·49	·178	·061	·128	2·14	·045	16·99	·357
17	413	12	11·87	35·14	·832	7·19	·170	·043	·103	2·12	·050	14·55	·345
18	402	10	5·26	39·12	·950	7·81	·190	·046	·112	3·91	·095	18·48	·449
19	441	4	7·58	39·16	1·153	7·37	·217	·053	·159	3·72	·109	17·12	·504
20	479	18	2·83	35·	·870	7·19	·178	·044	·109	1·75	·043	13·87	·344
21	317	3	3·08	41·53	·874	8·56	·180	·057	·122	1·85	·039	16·23	·341
22	313	12	10·12	40·18	·923	7·92	·192	·059	·144	1·73	·042	16·13	·391
23	374	14	9·01	39·65	·859	8·22	·178	·060	·131	3·11	·067	18·28	·396
24	507	14	3·84	35·47	·718	7·45	·151	·047	·99	2·87	·058	15·84	·322
25	356	13	11·26	44·10	·859	9·11	·177	·052	·101	3·41	·066	19·56	·380
26	209	11	5·44	55·	1·401	10·59	·269	·069	·177	2·20	·056	22·10	·562
27	448	2	8·38	37·68	1·023	7·05	·191	·048	·132	1·99	·054	14·67	·398
28	707	4	4·97	38·10	1	7·17	·188	·036	·95	1·78	·046	13·95	·366
29	402	9	4·43	41·72	1·038	7·94	·197	·060	·150	2·22	·051	16·13	·401
30	427	6	4·14	33·20	1·065	6·80	·218	·045	·146	1·75	·056	13·48	·432
31	439	7	5·52	36·53	·792	7·54	·169	·053	·116	6·10	·132	20·54	·445
32	432	7	2·30	39·76	·832	8·22	·173	·056	·119	2·43	·052	16·50	·349
33	385	5	2·16	40·64	·942	7·62	·177	·050	·118	3·94	·091	18·39	·428
34	431	6	3·58	39·36	1·026	7·32	·191	·067	·173	1·74	·045	15·58	·406
35	361	2	3·48	41·40	·825	7·76	·154	·056	·112	3·73	·074	18·51	·368
36	559	3	8·83	36·88	·863	7·	·164	·058	·137	2·33	·054	15·21	·356
	16,113	1	7·44	37·78	·872	7·49	·173	·053	·121	2·12	·049	15·27	·352

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITURE, from 1st January to 30th June 1839, inclusive—(*continued*).

MERCHANDIZE ENGINES.

Number of Engine.	Miles Run.	Tons conveyed 1 Mile.	COKE.				OIL.		Hose Pipes
			London (Good).	Birmingham (Inferior).	Total.	Cost.	Quarts.	Cost.	
			Cwts	Cwts.	Cwts.	£. s. d.		£. s. d.	£. s. d.
61	1900	173,778	720	91	811	77 13 9	56	2 6 8	0 9 6
62	5910	507,306	1658	514	2172	197 17 0	181½	7 11 3	0 16 6
63	3562	275,362	985	399	1384	123 8 9	133	5 10 10	0 10 6
64	2048	221,172	717	142	859	80 11 6	62½	2 12 1	..
65	5262	381,068	1432	591	2023	180 2 9	162	6 15 0	1 13 0
66	5658	503,694	1708	596	2304	209 1 0	159½	6 12 11	1 13 0
67	1641	211,756	595	61	656	63 6 3	49	2 0 10	0 19 0
68	2280	118,800	613	200	813	73 16 0	66½	2 15 5	..
69	680	182,392	885	158	1043	98 7 6	81	3 7 6	..
70	7678	484,364	2045	648	2693	245 4 6	217	9 0 10	0 16 6
71	4380	194,400	967	408	1375	122 9 10	96	4 0 0	..
72	1980	93,210	443	188	631	56 7 7	44½	1 17 1	..
73	1260	57,600	301	125	426	37 18 3	35	1 9 2	..
79	2115	171,824	914	239	1153	106 6 9	61	2 10 10	0 16 6
80	3875	371,260	1521	152	1673	161 10 0	114	4 15 0	..
81	2957	312,516	1000	124	1124	107 15 0	82	3 8 4	1 1 0
82	3046	359,758	1142	135	1277	122 12 9	83½	3 9 7	0 10 6
83	2398	240,408	956	76	1032	100 7 0	69	2 17 6	0 19 0
84	4808	434,432	1718	187	1905	183 9 9	118	4 18 4	0 10 6
	65438	5,295,100	20,320	5034	25,354	2348 5 11	1871	77 19 2	10 15 6

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITU

MERCHANDI

Number of Engine.	Fire Tools.	Enginemen's and Firemen's Wages.	REPAIRS. Engine.	REPAIRS. Tender.	Files.	Summary.	Proportion of General Charg
	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d
61	..	11 9 2	34 6 4·07	17 14 1·86	0 15 3	144 14 9·93	45 0 5
62	..	29 18 7½	116 8 6·93	1 19 8·50	1 9 10	356 1 5·93	110 15 1
63	0 8 0	30 14 2	38 11 4·10	2 17 0	1 17 4	203 17 11·10	63 8 5
64	..	17 1 3	23 5 1·92	1 12 7·80	0 1 0	125 3 7·72	38 18 9
65	..	31 18 2	85 18 3·99	2 11 10	1 0 10	309 19 10·99	96 8 5
66	0 9 0½	28 14 2	33 19 7·45	2 10 6	1 17 3	284 17 5·95	88 12 2
67	..	9 11 5	15 10 0·85	1 7 4·50	0 5 6	93 0 5·35	28 18 8
68	..	11 7 6	9 1 4·25	1 1 9	0 2 6	98 4 6·25	30 11 0
69	..	14 11 3	5 10 5·08	0 16 8·50	0 1 5	122 14 9·58	38 3 6
70	0 9 4	37 18 4	26 14 8·40	1 16 4	0 19 10	323 0 4·40	100 9 6
71	..	23 5 10	16 16 9·12	1 12 10·50	0 14 10	169 0 1·62	52 11 4
72	..	10 5 10	36 14 0·15	0 15 2	..	105 19 8·15	32 19 3
73	0 3 2	2 3 4	1 9 0·87	..	0 3 5	43 6 4·87	13 9 5
79	0 4 8	14 14 4	21 8 0·63	0 4 8·50	0 2 4	146 8 2·13	45 10 9
80	..	25 5 4	29 16 1·71	1 6 10·75	0 1 5	222 14 9·46	69 5 8
81	0 5 6	17 10 9	13 1 9·93	..	0 4 4	143 6 8·93	44 11 8
82	0 5 6	16 16 5	20 18 8·12	..	1 1 0	165 14 5·12	51 10 11
83	..	11 15 5	8 18 4·46	0 2 6	0 3 10	125 3 7·46	38 18 9
84	0 1 8	27 2 5	20 8 6·85	0 16 6·32	0 13 10	238 1 7·17	74 1 1
	2 6 10½	372 3 8½	558 17 4·88	39 6 8·23	11 15 9	3421 11 0·11	4887 3 1
						Fractional Differences	0 2 4
							4887 5 6
						Aylesbury Railway, as per Statement, p. 324	..
						Total Expenditure chargeable to the working of the Locomotive Engines	..

from 1st January to 30th June 1839, inclusive—(*continued*).

ENGINES.

Total Cost.			AVERAGE OF COKE.				AVERAGE OF OIL.		COST OF REPAIRS.		TOTAL COST.	
			Weight.		Cost.							
			Per Mile Run.	Per Ton per Mile.	Per Mile Run.	Per Ton per Mile.	Per Mile Run.	Per Ton per Mile.	Per Mile Run.	Per Ton per Mile.	Per Mile Run.	Per Ton per Mile.
£.	s.	d.	lbs.	lbs.	d.	d.	Pints.	Pints.	d.	d.	d.	d.
189	15	3·22	47·80	·522	9·81	·107	·058	·64	6·57	·071	23·97	·262
466	16	7·76	41·16	·479	8·03	·093	·061	·71	4·80	·056	18·95	·220
267	6	4·44	43·51	·563	8·31	·107	·074	·96	2·79	·036	18·	·233
164	2	4·86	46·97	·435	9·44	·087	·061	·56	2·91	·027	19·23	·178
406	8	4·92	43·05	·594	8·21	·113	·061	·85	4·07	·055	18·53	·255
373	9	8·53	45·60	·512	8·86	·099	·056	·63	1·54	·017	15·84	·177
121	19	1·71	44·77	·346	9·25	·071	·059	·46	2·46	·019	17·83	·138
128	15	7·05	39·98	·766	7·76	·149	·058	·112	1·06	·020	13·55	·260
160	18	4·40	43·58	·640	8·81	·129	·060	·89	·56	·008	14·41	·211
423	9	10·49	39·28	·622	7·66	·121	·056	·89	·89	·014	13·23	·209
221	11	6·37	35·16	·792	6·71	·151	·043	·99	1·01	·022	12·14	·273
138	19	0·10	35·69	·759	6·84	·145	·050	·95	4·54	·096	16·84	·357
56	15	10·83	37·86	·829	7·22	·158	·055	·121	·27	·006	10·81	·236
191	18	11·90	61·05	·752	12·06	·148	·059	·71	2·45	·032	21·78	·268
292	0	5·49	48·35	·504	10·	·104	·058	·61	1·92	·020	18·08	·188
187	18	5·39	41·32	·402	8·74	·082	·055	·52	1·06	·010	15·25	·144
217	5	4·64	46·95	·397	9·95	·081	·054	·46	1·01	·008	17·11	·145
164	2	4·51	48·20	·480	10·04	·100	·057	·57	·90	·008	16·42	·164
312	2	8·40	44·39	·491	9·15	·101	·049	·52	1·04	·011	15·57	·172
20,598	18	2·45	43·39	·536	8·61	·106	·057	·70	2·19	·027	16·44	·203
0	2	4·40										
20,599	0	6·85										
53	15	2·93										
20,652	15	9·78										

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITURE, from 1st January to 30th June 1839, inclusive—(*continued*).

ACCOUNT OF GENERAL CHARGES, included in the foregoing Statement.

		£. s. d.	£. s. d.
Pumping Engine at Camden		..	124 8 0·59
" Tring		..	50 16 8·05
Accident Account		..	81 15 8·03
Repairs of Implements		..	42 17 2
Pumping Engine at Rugby		..	9 11 8·50
" Coventry		..	3 2 5
" Beechwood		..	0 8 3
Repairs of Wheels and Axles		..	167 8 0
" Springs		..	80 10 0
General Charges, Coals and Firewood, London	£199 0 11		
Birmingham	211 11 4		
Wolverton	17 9 3		
		428 1 6	
Waste and Oil, London	69 2 11·25		
Birmingham	47 7 4		
Wolverton	25 6 4		
		141 16 7·25	
Cleaners' Wages, London	716 5 8		
Birmingham	859 15 6		
Wolverton	545 1 10		
		2121 3 0	
Painting, London	62 2 6·75		
Birmingham	73 15 3·25		
		135 17 10	
Stationery		57 15 1	
Clerks, Foremen, Watchmen, Office, and Sundry Expenses		1441 13 6·84	
			4326 7 7·09
		£	4887 5 6·26

ACCOUNT OF EXPENDITURE, AYLESBURY RAILWAY.

	£ s. d.
Coke	30 0 0
Oil	2 16 8
Hose Pipes	2 5 0
Engineman and Fireman's Wages	11 19 7
Repairs of Engine	4 6 3·93
Files, Oil Cans, Cleaning, &c.	2 7 8
	£53 15 2·93

	£. s. d.
Amount chargeable to the working Locomotive Engines	20,652 15 9·78
Stationary Engine for working Euston Square Incline, and sundry Expenses not chargeable to the working of the Locomotive Engines	2,526 7 3·22
Total Locomotive Expenditure	£23,179 3 1

1*st August*, 1839. EDWARD BURY.

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITURE,

From 1st July to 15th December 1839, inclusive.

PASSENGER ENGINES.

Number of Engine.	From the Opening of the Line to the 30th June 1839.		Miles Run from 1st July 1839 to 31st December 1839.	Tons conveyed 1 Mile.	Time of Performance.		COKE.					
							Weight.			Cost.		
	Total Miles Run.	Total Cost of Repairs.					London (Good).	Birmingham (Inferior).	Total.			
		£ s. d.			h.	m.	Cwts.	Cwts.	Cwts.	£.	s.	d.
1	27527	260 4 2	7059	342,224	293	58	2151	227	2378	222	8	3
			1904*									
2	20222	149 16 5	5533	219,686	233	29	1529	142	1671	161	15	6
			192*									
3	14895	103 18 0	4977	215,948	211	10	1555	165	1720	165	16	3
4	14550	102 7 11	10563	515,813	428	16	3391	283	3674	357	10	9
5	17597	120 10 6	6960	292,483	272	22	1626	533	2159	202	4	1
6	16265	134 17 8	6185	279,150	256	22	1043	958	2001	179	6	7
7	25604	206 2 3	6328	316,454	264	47	1833	451	2284	212	4	9
8	19228	114 17 1	6591	323,959	323	47	1231	709	1940	174	7	10
9	17073	106 8 7	4500	249,252	194	18	1070	371	1441	133	0	2
10	24019	253 16 11	5138	247,754	202	9	1200	339	1539	145	13	0
11	18915	184 13 11	7579	331,892	325	57	2254	353	2607	247	9	3
12	14481	131 1 3	3711	204,435	170	24	766	1014	1780	160	5	0
13	18900	130 10 9	7044	324,180	297	8	1300	1159	2459	224	14	9
14	12206	81 7 10	7755	406,545	288	39	1825	240	2065	207	10	0
15	15198	100 4 11	8036	338,839	334	2	2493	144	2637	258	6	0
			112*									
16	15138	133 8 9	7804	339,180	338	7	2249	252	2501	240	13	0
17	21136	185 14 5	2109	103,891	89	10	721	59	780	75	15	9
			5776*									
18	18992	205 12 4	7606	275,255	326	26	2459	260	2719	270	5	6
			256*									
19	17386	193 18 6	6634	284,853	275	45	1887	809	2696	245	4	6
20	20780	169 19 6	5973	256,429	262	24	1846	155	2001	194	5	9
21	16357	138 7 10	7294	341,283	341	59	2312	230	2542	245	11	6
			336*									
22	13313	109 8 6	153	13,900	9	11	69	21	90	8	4	3
23	14913	237 5 6	3937	222,159	193	12	1314	162	1476	141	10	6
			3152*									
24	21662	214 9 0	7217	325,515	329	55	2389	286	2675	256	15	6
25	21136	208 18 11	5590	266,566	261	14	2052	113	2165	212	17	9
			112*									
26	5164	46 2 9	9745	342,005	387	50	1990	1080	3070	281	8	3
27	15575	133 1 0	..	..	..		..	..	..	..		
28	21531	172 3 7	4423	179,730	185	3	1122	656	1778	168	6	0
29	12172	110 13 7	5277	229,578	210	25	1462	641	2103	192	1	2
30	15964	128 17 3	5907	278,466	248	34	1893	336	2229	210	15	9
31	19494	255 3 9	8401	368,984	367	38	2615	226	2841	275	12	6
32	16105	150 5 11	6987	300,394	311	58	2239	156	2395	233	13	0
33	13691	158 6 6	6640	282,327	270	29	1450	1198	2648	238	14	10
34	16888	136 19 6	5375	236,545	218	28	1007	895	1902	169	14	9
35	9364	113 17 10	3360	268,190	183	42	790	547	1337	117	14	8
36	18568	171 5 7	5225	252,084	214	2	1210	857	2067	183	3	2
37	..	..	4182	197,637	157	41	764	637	1401	134	4	11
38	..	..	3240	140,398	113	21	581	344	925	91	16	7
39	..	..	1692	79,169	73	2	611	65	676	65	16	9
	622009	5554 15 8	222,730	10,193,152	9466	24	60,299	17,073	77,372	7306	18	6

* Aylesbury.

London and Birmingham Railway Locomotive Expenditure, from 1st July to 15th December 1839, inclusive—(*continued*).

PASSENGER ENGINES.

Number of Engine.	Oil. Quarts.	Oil. Cost. £. s. d.	Hose Pipes. £. s. d.	Fire Tools. £. s. d.	Enginemen's and Firemen's Wages. £. s. d.	Repairs. Engine. £. s. d.	Repairs. Tender. £. s. d.	Files. £. s. d.
1	167	6 19 2	0 19 0	..	39 16 7	167 4 11½	1 0 1½	0 9 9
2	144	6 0 0	1 18 0	..	33 3 2	111 16 3½	11 13 1	3 1 11
3	130½	5 8 9	3 4 0	0 4 0	29 11 4	52 16 2	8 16 2½	1 1 1
4	232½	9 13 9	1 18 0	0 4 0	64 4 8	57 10 2½	3 17 2	1 16 10
5	251	10 9 2	..	0 4 8	48 10 6	61 6 0½	2 10 7	1 2 3
6	211½	8 16 3	1 13 0	..	44 18 8½	37 12 4½	16 13 5	1 19 6
7	219½	9 2 11	0 16 6	0 4 0	39 8 5	143 9 3½	2 0 9½	3 4 0
8	202	8 8 4	0 16 6	0 4 0	50 17 8	68 4 2	11 11 2	4 11 9
9	135	5 12 6	1 13 0	..	23 4 2	35 18 2	3 3 6	1 7 1
10	147	6 2 6	3 8 6	0 4 0	27 9 2½	93 14 11½	6 15 0	1 3 3
11	174	7 5 0	1 6 0	0 4 0	54 4 4½	67 9 1	4 6 0	2 10 9
12	243½	10 2 11	2 9 6	..	50 19 8½	38 3 7½	3 8 3½	1 4 3
13	236½	9 17 1	..	..	49 5 8	66 8 6½	10 19 7½	0 19 0
14	147½	6 2 11	1 13 0	..	38 3 4	71 0 3	2 6 9	1 5 0
15	167	6 19 2	0 9 6	..	45 13 10	55 17 10½	3 1 10½	1 16 10
16	211	8 15 10	..	0 4 0	53 11 5½	41 5 8½	8 17 5½	1 1 4
17	58	2 8 4	0 19 0	..	15 16 9½	26 13 5½	6 2 2	0 15 9
18	219½	9 2 11	3 4 6	0 4 0	51 7 9½	44 18 7	4 13 0½	1 10 1
19	221	9 4 2	0 19 0	..	58 5 6½	61 15 9½	24 17 0½	0 5 5
20	164½	6 17 1	0 16 6	..	36 13 6½	47 0 10	7 17 7½	1 16 1
21	197½	8 4 7	0 19 0	..	41 9 4½	80 6 10	3 9 10	1 6 6
22	4	0 3 4	0 19 0	..	1 1 8	90 6 3½	1 3 2	0 17 11
23	108	4 10 0	2 7 6	..	36 6 8	74 12 8	4 16 3½	1 14 2
24	206½	8 12 1	3 4 0	0 4 0	50 13 0½	56 7 5	2 19 6½	1 14 7
25	179½	7 9 7	2 7 6	0 4 0	44 3 7	90 0 8½	8 19 8½	2 0 4
26	280½	11 13 9	1 6 0	0 1 10	58 5 11	32 1 4	0 17 2	0 9 6
27	..	..	..	..	..	62 13 5½	10 11 0½	3 0 2
28	181	7 10 10	..	..	51 10 7	79 5 6½	8 9 8½	1 4 4
29	190½	7 18 9	0 9 6	..	54 15 7	77 4 10	9 6 4	0 13 10
30	142½	5 18 9	1 18 0	0 4 0	43 13 1½	58 18 10	27 7 2½	1 8 11
31	234	9 15 0	7 7 3	0 8 0	52 2 0	48 1 1½	5 4 6	1 7 9
32	187	7 15 10	3 1 6	0 4 0	42 8 5½	87 2 0	4 4 3½	1 6 0
33	242	10 1 8	2 5 0	0 8 0	66 3 0½	80 9 0	7 11 6	2 11 4
34	215½	8 19 7	1 13 0	0 4 0	56 10 9½	82 4 10½	11 5 8½	3 4 4
35	138	5 15 0	..	0 4 0	36 8 0½	42 4 7½	1 17 7	1 14 7
36	242½	10 2 11	1 13 0	0 4 0	53 7 10½	69 18 0½	1 10 1½	2 16 6
37	177	7 7 6	0 19 0	0 4 0	19 8 10	16 0 11	0 10 9	0 6 8
38	106	4 8 4	..	..	11 3 4	15 0 5½	2 16 10½	0 4 5
39	49	2 0 10	..	0 4 0	8 15 10	3 5 0½	0 7 5	..
	6763½	*281 17 1*	58 12 9	4 10 6	1583 14 2½	2496 10 4½	257 19 8	61 3 9

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITURE, from 1st July to 15th December 1839, inclusive—(*continued*).

PASSENGER ENGINES.

Number of Engine.	Summary.	Proportion of General Charges.	Total Cost.	Average of Coke. Weight. Per Mile Run.	Average of Coke. Weight. Per Ton per Mile.	Average of Coke. Cost. Per Mile Run.	Average of Coke. Cost. Per Ton per Mile.	Cost of Repairs. Per Mile Run.	Cost of Repairs. Per Ton per Mile.	Total Cost. Per Mile Run.	Total Cost. Per Ton per Mile.
	£. s. d.	£. s. d.	£. s. d.	lbs.	lbs.	d.	d.	d.	d.	d.	d.
1	438 17 10	149 15 4	588 13 2	37·73	·778	7·56	·155	5·72	·118	18·45	·383
2	329 7 11½	112 8 11	441 16 10½	33·62	·851	7·	·176	5·35	·135	19·16	·482
3	266 17 9½	91 1 3	357 19 0½	38·70	·892	7·99	·178	2·97	·068	17·26	·397
4	496 15 4½	169 10 0	666 5 4½	38·95	·797	8·12	·166	1·39	·028	15·14	·310
5	326 7 3½	111 7 1	437 14 4½	34·74	·826	6·97	·160	2·20	·052	15·09	·359
6	290 19 10	99 5 10	390 5 8	36·23	·803	6·95	·172	2·10	·046	15·14	·335
7	410 10 8	140 1 8	550 12 4	40·42	·808	8·04	·160	5·51	·110	20·88	·417
8	319 1 5	108 17 4	427 18 9	31·44	·670	6·35	·129	2·89	·059	15·58	·317
9	203 18 7	69 11 7	273 10 2	36·18	·647	7·15	·123	2·10	·037	14·58	·263
10	284 10 5	97 1 8	381 12 1	33·54	·695	6·80	·141	4·69	·097	17·82	·369
11	384 14 5½	131 5 4	515 19 9½	38·52	·879	7·83	·178	2·27	·052	16·33	·373
12	266 13 3½	90 19 8	357 12 11½	53·72	·975	10·36	·180	2·69	·048	23·13	·419
13	362 4 8	123 11 11	485 16 7	39·09	·849	7·65	·166	2·63	·056	16·55	·359
14	328 1 3	111 18 9	440 0 0	29·81	·569	6·39	·124	2·27	·043	13·61	·259
15	372 5 1	127 0 3	499 5 4	36·75	·871	7·71	·179	1·76	·041	14·91	·353
16	354 8 9½	120 18 8	475 7 5½	35·89	·825	7·38	·170	1·54	·035	14·62	·307
17	128 11 3	43 17 4	172 8 7	41·42	·840	8·62	·175	3·73	·075	19·62	·398
18	385 6 5	131 9 6	516 15 11	40·03	1·106	8·52	·235	1·56	·043	16·30	·479
19	400 11 5½	136 13 6	537 4 11½	45·51	1·060	8·87	·206	3·13	·073	19·45	·452
20	295 7 5	100 15 11	396 3 4	37·52	·870	7·80	·181	2·20	·051	15·91	·370
21	381 7 7½	130 2 7	511 10 2½	39·16	·834	8·08	·107	2·75	·058	16·81	·359
22	102 15 7½	35 2 2	137 17 9½	65·88	·725	12·81	·141	143·10	1·579	216·29	2·380
23	265 17 9½	90 14 6	356 12 3½	41·99	·744	8·62	·152	4·84	·085	21·73	·389
24	380 10 2	129 16 8	510 6 10	41·51	·920	8·53	·189	1·97	·043	16·97	·376
25	368 3 2	125 12 4	493 15 6	43·37	·902	9·14	·191	4·25	·089	21·20	·444
26	386 3 9	131 15 4	517 19 1	35·28	1·005	6·93	·197	0·81	·023	12·75	·363
27	76 4 8	26 0 2	102 4 10								
28	316 7 0	107 18 9	424 5 9	45·	1·108	9·13	·224	4·76	·117	23·00	·566
29	342 10 0	116 17 3	459 7 3	44·63	1·026	8·73	·206	3·93	·091	20·89	·480
30	350 4 7	119 10 0	469 14 7	42·26	·896	8·56	·181	3·50	·074	19·08	·404
31	399 18 1½	136 9 0	536 7 1½	37·87	·862	7·87	·179	1·52	·034	15·32	·349
32	379 15 1	129 11 5	509 6 6	38·39	·892	8·02	·186	3·13	·072	17·49	·406
33	408 4 4½	139 5 9	547 10 1½	44·66	1·050	8·64	·202	3·18	·075	19·79	·465
34	333 17 0½	113 18 4	447 15 4½	39·63	·900	7·59	·172	4·17	·095	19·99	·454
35	205 18 6	70 5 3	276 3 9	44·56	·558	8·40	·105	3·15	·039	19·72	·247
36	322 15 7½	110 2 8	432 18 3½	44·30	·918	8·41	·174	3·28	·068	19·88	·412
37	179 2 7	61 2 5	240 5 0	37·54	·794	7·70	·162	0·95	·020	13·79	·291
38	125 10 0	42 16 5	168 6 5	31·97	·738	6·79	·156	1·32	·030	12·47	·287
39	80 9 10½	27 9 3	107 19 1½	44·74	·956	9·33	·198	0·51	·011	15·30	·327
	12,051 6 10	4112 1 9	16,163 8 7	38·90	0·850	7·87	·172	2·96	·064	17·41	·380

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITURE, from 1st July to 15th December 1839, inclusive—(*continued*).

MERCHANDIZE ENGINES.

Number of Engine.	From the Opening of the Line to the 30th of June 1839. Total Miles Run.	Total Cost of Repairs. £. s. d.	Miles Run from 1st July 1839 to 15th December 1839.	Tons conveyed 1 Mile.	Time of Performance. h. m.	COKE. Weight. London (Good). Cwts.	Birmingham (Inferior). Cwts.	Total. Cwts.	Cost. £. s. d.
61	3184	63 5 2	7641	425,966	321 13	1896	745	2641	243 13 6
62	6630	124 14 3	6624	630,927	381 33	1691	998	2689	244 18 4
63	3802	43 10 4	4482	446,992	261 24	1083	723	1806	164 12 1
64	2048	24 17 10	7614	488,971	349 3	2582	264	2846	274 14 0
65	5262	88 10 2	7320	321,415	307 0	1512	1103	2615	227 3 10
66	5658	36 10 1	6084	614,419	353 44	1379	876	2255	200 7 9
67	1641	16 17 5	5208	554,244	347 16	1839	246	2085	199 5 6
68	2280	10 3 1	5172	245,291	214 1	1170	590	1760	159 6 8
69	2680	6 7 1	8238	495,130	365 42	2792	315	3107	298 17 9
70	7678	28 11 0	12042	592,525	488 19	2685	1225	3910	359 9 4
71	4380	18 9 7	10311	531,205	409 29	2039	1043	3082	286 16 4
72	1980	37 9 2	8184	379,337	323 5	1744	1146	2890	254 11 3
73	1260	1 9 1	5493	268,375	223 33	1241	786	2027	183 8 4
74	..	..	6840	683,443	395 52	1523	1235	2758	249 13 0
75	..	..	5586	285,036	233 48	1206	798	2004	178 8 6
76	..	..	4755	272,429	259 0	1597	211	1808	172 17 9
77	..	..	4980	504,095	277 25	1205	1178	2383	210 3 11
78	..	..	786	41,664	36 16	204	167	371	34 11 9
79	11607	55 19 11	6010 112*	353,288	270 46	2039	166	2205	214 5 6
80	11069	46 6 5	1617 10300†	81,188	69 8	457	158	615	55 11 6
81	7664	18 8 11	3774 8400†	403,200	253 7	1245	140	1385	133 5 0
82	4474	33 8 8	5966	692,567	388 46	2137	251	2388	229 7 9
83	3448	9 0 10	7362	720,523	438 50	2512	346	2858	273 13 2
84	4808	21 5 1	6879	509,146	315 57	2703	295	2998	288 14 9
85	..	..	9618	506,880	400 45	2424	972	3396	315 8 3
86	..	..	7041	359,843	300 35	1453	834	2287	210 8 6
87	..	..	3783	404,016	253 38	1472	177	1649	158 5 3
88	..	..	2043 1500†	229,784	139 13	851	62	913	88 19 6
89	..	..	1704	133,432	99 3	269	401	670	63 8 8
90	..	..	561	57,008	37 24	175	51	226	20 13 9
	91553	685 4 1	173,718	12232,339	8514 55	47,125	17,502	64,627	5995 1 2
Total of Passengers and Goods Engines.	713562	6239 19 9	428,600 including Aylesbury and Ballasting. 396,448	22,425,491	17,981 19	107,424	34,575	141,999	13,301 19 8
		Passenger Trains	326,681	5,176,845	13,596 59				
		Merchandize Trains	69,767	7,248,646	4,384 20				
			396,448	22,425,491	17,981 19				

* Aylesbury. † Ballasting.

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITURE, from 1st July to 15th December 1839, inclusive—(*continued*).

MERCHANDIZE ENGINES.

Number of Engine.	Oil. Quarts.	Oil. Cost. £. s. d.	Hose Pipes £. s. d.	Fire Tools. £. s. d.	Enginemen's and Firemen's Wages. £. s. d.	Repairs. Engine. £. s. d.	Repairs. Tender. £. s. d.	Files. £. s. d.
61	250½	10 8 9	0 9 6	0 4 0	43 1 9	63 10 0½	4 1 3½	2 4 5
62	210	8 15 0	5 15 6	..	29 0 10	111 9 7	19 2 6½	1 15 11
63	134	5 11 8	0 16 6	0 9 0	24 10 9	43 3 5	18 0 10½	1 2 3
64	219	9 2 6	0 19 0	..	45 4 10	39 5 7½	10 0 8½	0 14 4
65	213	8 17 6	2 19 0	..	42 0 6	50 19 1½	1 7 1	1 6 5
66	163½	6 16 3	..	..	29 1 6	45 3 8	3 6 1	2 1 3
67	153½	6 7 11	..	..	29 2 11½	84 0 11½	4 5 9½	2 6 3
68	140½	5 17 1	1 13 0	..	27 6 10	29 2 1	0 10 6	0 13 3
69	224	9 6 8	..	..	43 19 4	37 13 7½	5 15 3	0 15 7
70	359	14 19 2	2 12 0	..	56 2 1½	52 0 1	4 5 1½	1 14 1
71	277	11 10 10	4 2 6	0 4 0	49 0 0	43 17 10	3 6 2½	1 17 7
72	251	10 9 2	2 19 0	..	39 11 1½	47 0 5½	2 6 4½	0 15 9
73	187	7 15 10	2 2 6	0 4 0	31 7 1	77 5 2	5 10 8	2 10 6
74	245½	10 4 7	0 16 6	..	37 15 0	28 13 7	3 7 3	1 14 9
75	222	9 5 0	0 16 6	0 4 0	24 19 2	30 11 8	0 11 2½	1 2 0
76	144	6 0 0	1 18 0	0 4 0	26 14 4	110 5 8	2 12 2½	2 7 5
77	193	8 0 10	1 13 0	..	25 2 1	36 5 0½	2 5 7	1 1 1
78	46	1 18 4	..	..	3 9 10	3 13 11	0 2 0	0 7 0
79	177½	7 7 11	0 19 0	..	33 17 6½	70 12 10½	0 1 1	1 9 2
80	61½	2 11 3	..	..	13 7 1	81 17 9	..	0 17 1
81	110	4 11 8	0 19 0	0 8 0	20 2 11	33 3 2	..	1 6 3
82	142½	5 18 9	..	..	30 10 4	58 12 0	0 16 0½	1 14 9
83	194½	8 2 1	2 2 6	..	37 4 5	38 16 4½	0 3 9	0 9 4
84	206½	8 12 1	2 5 0	..	39 8 10½	50 2 8½	18 14 11½	1 3 3
85	325½	13 11 3	0 16 6	0 4 0	45 15 2	16 9 6	0 0 6	1 0 2
86	182½	7 12 1	0 4 0	..	30 5 2	15 19 8	0 12 6	0 5 4
87	121½	5 1 3	0 9 6	..	19 0 0	19 10 11	0 2 1½	0 6 0
88	71½	2 19 7	0 19 0	0 5 0	13 10 9	20 12 11½	..	0 8 0
89	54	2 5 0	..	..	7 6 6	5 3 0½	0 3 6	0 2 6
90	31	1 5 10	..	..	2 14 2	1 7 0½	..	..
	5311	221 5 10	38 7 0	2 6 0	900 12 11½	1346 9 6½	111 11 2½	35 11 8
Total of Passengers and Goods Engines.	12,074½	503 2 11	96 19 9	6 16 6	2484 7 2	3842 19 11	369 10 10½	96 15 5

London and Birmingham Railway Locomotive Expenditure, from 1st July to 15th December 1839, inclusive—(*continued*).

MERCHANDIZE ENGINES.

Number of Engine.	Summary.	Proportion of General Charges.	Total Cost.	Average of Coke. Weight. Per Mile Run.	Average of Coke. Weight. Per Ton per Mile.	Average of Coke. Cost. Per Mile Run.	Average of Coke. Cost. Per Ton per Mile.	Cost of Repairs. Per Mile Run.	Cost of Repairs. Per Ton per Mile.	Total Cost. Per Mile Run.	Total Cost. Per Ton per Mile.
	£. s. d.	£. s. d.	£. s. d.	lbs.	lbs.	d.	d.	d.	d.	d.	d.
61	367 13 3	125 9 0	493 2 3	38·71	·694	7·65	·137	2·12	·037	15·48	·277
62	420 17 8½	143 12 2	564 9 10½	45·46	·477	8·87	·093	4·73	·049	20·45	·214
63	258 6 6½	88 2 10	346 9 4½	45·17	·452	8·81	·088	3·32	·032	18·55	·186
64	380 1 0	129 13 6	509 14 6	41·86	·652	8·65	·134	1·46	·024	16·06	·249
65	334 13 5½	114 3 10	448 17 3½	40·	·911	7·44	·169	1·71	·039	14·71	·335
66	286 16 6	97 17 4	384 13 10	41·57	·411	7·90	·079	1·91	·019	15·17	·150
67	325 9 4½	111 1 0	436 10 4½	44·83	·421	9·18	·086	2·15	·038	20·11	·189
68	224 9 5	76 11 8	301 1 1	38·11	·803	7·39	·155	1·37	·029	13·97	·294
69	396 8 2½	135 5 1	531 13 3½	42·24	·703	8·70	·145	1·26	·021	15·49	·257
70	491 1 11	167 11 3	658 13 2	36·36	·739	7·16	145	1·12	·022	13·12	·266
71	400 15 3½	136 14 10	537 10 1½	33·47	·649	6·67	·129	1·09	·021	12·51	·395
72	357 13 1½	122 0 8	479 13 9½	39·55	·853	7·46	·161	1·44	·031	14·06	·306
73	310 4 1	105 16 10	416 0 11	41·33	·846	8·	·164	3·63	·074	18·16	·372
74	332 4 8	113 7 2	445 11 10	45·16	·463	8·76	·087	1·12	·011	15·63	·154
75	245 18 0½	83 18 0	329 16 0½	40·18	·787	7·66	·150	1·33	·026	13·98	·277
76	322 19 4½	110 3 11	433 3 3½	42·58	·743	8·72	152	5·69	·099	21·86	·382
77	284 11 6½	97 1 11	381 13 5½	53·59	·529	10·13	·100	1·85	·018	18·39	·179
78	44 2 10	15 1 2	59 4 0	52·86	·997	10·56	·199	1·15	·021	18·07	·241
79	328 13 1	112 2 9	440 15 10	41·09	·699	8·55	·145	2·82	·048	17·60	·299
80	154 4 8	52 12 6	206 17 2	42·59	·848	8·24	·164	12·15	·242	30·70	·611
81	193 16 0	66 2 6	259 18 6	41·10	·384	8·47	·079	2·03	·019	16·53	·154
82	326 19 7½	111 11 4	438 10 11½	44·83	·386	9·23	·079	2·38	·020	17·64	·152
83	360 11 7½	123 0 7	483 12 2½	43·48	·444	8·92	·091	1·27	·013	15·76	·162
84	409 1 7½	139 11 7	548 13 2½	48·86	·659	10·07	·136	2·40	·032	19·14	·259
85	393 5 4	134 3 8	527 9 0	39·54	·750	7·87	·149	0·41	·007	13·16	·249
86	265 7 3	90 10 10	355 17 1	36·37	·711	7·17	·140	0·56	·011	12·12	·237
87	202 15 0½	69 3 7	271 18 7½	48·82	·457	9·77	·094	1·24	·011	17·25	·161
88	127 14 9½	43 11 8	171 6 5½	50·05	·445	10·45	·093	2·42	·021	20·12	·178
89	78 9 2½	26 17 11	105 7 1½	44·03	·562	8·93	·114	0·74	·009	14·83	·189
90	26 0 9½	8 17 8	34 18 5½	45·11	·444	8·85	·087	0·57	·005	14·94	·147
	8651 4 4½	2951 18 9	11,603 3 1½	41·66	0·591	8·28	·117	2·01	·028	16·03	·227
Total of Passengers and Goods Engines.	20,702 12 2½	7064 0 6	27,766 12 8½	40·11	·709	8·05	·142	2·55	·045	16·80	·297
	504 19 7	172 2 0	677 1 7	Aylesbury Railway, as per Statement, p. 331.							
		7236 2 6									
		£	28,443 14 3½	Total Expenditure chargeable to the working of the Locomotive Engines							

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE EXPENDITURE, from 1st July to 15th December 1839, inclusive—(*continued*).

ACCOUNT OF GENERAL CHARGES included in the foregoing Statement.

	£. s. d.	£. s. d.	£. s. d.
Pumping Engines—Camden	88 13 0		
Tring	39 1 8		
Rugby	11 8 0		
Weedon	10 8 8		
Coventry	12 8 3		
Beechwood	7 4 6		
		..	169 4 1
Stationary Engine, Wolverton		..	20 3 10
Accident Account		..	556 17 9½
Implements and Repairs of ditto		..	595 12 7
Repairs of Wheels and Axles		..	93 1 0
„ Springs		..	40 1 0
Coals and Firewood—London		221 16 1½	
Birmingham		30 18 3	
Wolverton		226 8 3½	
			479 2 8
Stationery		..	114 16 1
Waste and Oil for Cleaners—London		95 19 0	
Birmingham		57 13 8	
Wolverton		85 5 5½	
			238 18 1½
Cleaners' Wages—London		836 6 2	
Birmingham		894 1 5½	
Wolverton		769 5 9½	
			2499 13 5
Painting—London		77 8 11½	
Birmingham		38 0 6½	
Wolverton		31 14 4	
			147 3 10
Clerks, Foremen, Watchmen, Office, and Sundry Expenses		..	2281 8 1
		£	7236 2 6

STATEMENT OF EXPENDITURE, AYLESBURY RAILWAY.

	£. s. d.
Coke, 3473 cwts.	347 6 0
Oil, 262 quarts	10 18 4
Hose Pipes	0 9 6
Engineman's and Fireman's Wages	108 14 9½
Repairs, Engine	7 8 0½
Repairs, Tender	0 4 0
Oil and Waste for Cleaners	2 6 0
Cleaner's Wages	23 16 11
One Signal Lamp, and fitting ditto	3 16 0
	£504 19 7

	Miles.
No. 1	1904
2	192
15	112
17	5776
18	256
21	336
23	3152
25	112
79	112
	11,952

		£. s. d.
Expenditure, as per above Statement		28,443 14 3½
Stationary Engine for working Euston Incline, and Sundry Expenses not chargeable to working of Locomotive Engines		1,787 1 9
Total Expenditure to 15th December		30,230 16 0½
Amount of Pay Bills for Wages for the fortnight, from the 14th to 31st December 1839	£1009 1 9	
Sundries not classed	6 1 3	1,015 3 0
		£31,245 19 0½

January 31st, 1840.

EDWARD BURY.

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE POWER ACCOUNT.

REPAIRS OF ENGINES AND TENDERS,
From the Commencement of their Running to the 14th June 1840.

	Total.	DIVIDED ACCORDING TO THOSE THAT HAVE RUN				
		Under 10,000 Miles.	Above 10 000 Miles.	Above 20,000 Miles.	Above 30,000 Miles.	Above 40,000 Miles.
Number of Locomotives	82	15	29	24	13	1
Total Number of Miles Run	1,635,396	89,266	444,421	629,833	429,944	41,932
Average Number of Miles Run per Engine	19,944	5,951	15,328	26,243	33,073	41,932
	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Total Cost of Repairs .	17346 9 8	529 8 9	4467 3 7	6905 15 0	4952 11 0	491 11 4
Average Cost of Repairs per Engine .	211 10 10	35 5 11	154 0 10	287 14 9	380 19 4	491 11 4
	d.	d.	d.	d.	d.	d.
Ditto ditto per Mile .	2·545	1·423	2·412	2·631	2·765	2·813
	£. s. d.	s. d.	£. s. d.	£. s. d.	£. s. d.	s. d.
Ditto ditto per Journey of 111 Miles . .	1 3 6	13 2	1 2 4	1 4 4	1 5 7	1 6
For the first 10,000 Miles, the Cost of Repairs, per Engine, and per Mile . .	d. 2					
Ditto 20,000 ditto, ditto	2·5					
Ditto 30,000 ditto, ditto	2·667					
Ditto 40,000 ditto, ditto	2·750					
For the Second 10,000 ditto, ditto . .	3					
For the Third 10,000 ditto, ditto . .	3					
For the Fourth 10,000 ditto, ditto . .	3					

LOCOMOTIVE POWER ACCOUNT,

From 15th December 1839 to 14th June 1840.

	Total.	DIVIDED ACCORDING TO ENGINES HAVING		
		Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Wheels, 5 ft. 6 in. Diameter; One Pair Driving Wheels.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in. Wheels, 5 ft. 6 in. Diameter; One Pair Driving Wheels.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in. Wheels. 5 ft. Diameter; Coupled Wheels.
Number of Locomotives	82	42	10	30
Ditto Miles Run since Opening of Railway to June 14, 1840	1,635,396	1,063,210	92,101	480,085
Average Number of ditto by each Engine.	19,944	25,315	9,210	16,003
Number of Miles Run from December 14, 1839, to June 14, 1840	474,154	215,745	82,987	175,422
Ditto, with Passenger Trains	383,858	207,400	82,747	93,711
Ditto, with Merchandize Trains	90,296	8,345	240	81,711
Total Number of Journeys of 111 Miles	4,272	1,944	748	1,580
Ditto with Passenger Trains	3,459	1,869	746	844
Ditto with Merchandize Trains	813	75	2	2
Greatest Number of Miles Run by any one Engine	. .	9,778	15,228	9,960
Total Number of Passengers conveyed one Mile (distance 112 Miles)*	21,582,872			
Ditto apportioned among the several Engines, according to Gross Load	. .	10,430,636	5,275,291	587,695
Total Number of Passengers conveyed one Mile (distance 111 Miles)	21,390,167			
Ditto apportioned among the several Engines	. .	10,337,505	5,228,190	5,824,672
Average Number of Passengers per each Journey	. .	50	63	62
WEIGHT CONVEYED.	Tons.	Tons.	Tons.	Tons.
Estimated Gross Weight of each Passenger† with his Luggage	0·551			
Ditto Net Weight ditto (2¼ cwt.)	0·1125			
Ditto ditto Horses (9 cwt.)	0·22222			
Ditto ditto Carriages	1·			
Ditto ditto Dogs (42 lbs.)	0·01873			
Ditto ditto Mail Bags, per diem (4 tons, 8 cwt. 2 qrs.)	4·425			
Ditto ditto Cattle (5½ cwt.)	0·275			
Ditto ditto Sheep (¾ cwt.)	0·0375			
Ditto ditto Pigs (1 cwt.)	0·05			
Gross Load ‡ conveyed one Mile	24257775·	8021312·	3765571·	12470872·
Ditto by Passenger Trains	15347832·	7417347·	3751321·	4179164·
Ditto by Merchandize Trains	8909943·	603965·	14250·	8291728·
Net Load estimated from the above weights	7028463·	1779221·	772606·	4776636·
Ditto by Passenger Trains	3135503·	1515335·	766380·	3135503·
Ditto by Merchandize Trains	3892960·	263886·	6226·	3892960·
Average Gross Load per each Journey of the Engine (111 Miles) by Passenger Trains	39,983	35,763	45,335	44,596
Ditto by Merchandize Trains	98,675	72,374	59,375	101,476

* The distance, according to the mileage duty, is 112 miles, but a portion of that distance, at the London end, being performed by a fixed engine, the distance run by the engine is only 111 miles.

† This includes the portion of the weight of the train due to one passenger.

‡ Gross load includes the weight of the carriages (5 tons each), but not the weight of the engine and tender.

LOCOMOTIVE POWER ACCOUNT,

From 15th December 1839 to 14th June 1840—(*continued*).

	Total.	DIVIDED ACCORDING TO ENGINES HAVING		
		Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Wheels, 5 ft. 6 in. Diameter; One Pair Driving Wheels.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in; Wheels, 5 ft. 6 in. Diameter; One Pair Driving Wheels.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Wheels 5 ft. Diameter; Coupled Wheels.
RATE OF SPEED.	Hours. Minutes.	Hours. Minutes.	Hours. Minutes.	Hours. Minutes.
Total Time the Passenger Trains were in Motion	15382 21	8385 59	3119 39	3876 43
Average ditto ditto, per each Journey	4 27	4 29	4 11	4 35½
	Miles per Hour.	Miles per Hour.	Miles per Hour.	Miles per Hour.
Average Speed of Passenger Trains	24·954	24·732	26·524	24·173
Ditto of Merchandize Trains	20	20	20	20
COKE CONSUMED.	Tons. cwts. qrs. lbs.	Tons. cwts. qrs. lbs.	Tons. cwts. qrs. lbs.	Tons. cwts. qrs. lbs.
Total Quantity	8405 8 0 0	3608 6 0 0	1298 2 0 0	3499 0 0 0
Quantity per Journey	1 19 1 11	1 17 0 14	1 14 2 25	2 4 1 3
Ditto per Hour	0 8 1 22	0 8 0 22	0 8 1 4	0 8 3 3
Ditto per Mile	0 0 1 11·709	0 0 1 9·464	0 0 1 7·039	0 0 1 16·679
Ditto per Ton of Gross Load per Mile	0 0 0 0·776	0 0 0 1·007	0 0 0 0·772	0 0 0 0·630
Ditto ditto per Journey	0 0 3 2	0 1 0 0	0 0 3 1	0 0 2 14
OIL CONSUMED.	Quarts.	Quarts.	Quarts.	Quarts.
Total Quantity	15675·5	6905·	2540·	6230·5
Quantity per Journey	3·670	3·552	3·397	3·936
Ditto per Mile	0·0331	0·032	0·0306	0·0354
COST OF COKE.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Total Cost, at an Average of 40*s.* per ton	16810 2 3	7216 2 9	2596 4 0	6997 15 6
Cost per Engine per Journey (111 Miles)	3 18 8	3 14 3	3 9 5	4 8 7
Ditto per Hour	0 16 11	0 16 5	0 16 7	0 17 7
Ditto per Mile	0 0 8·509	0 0 8·027	0 0 7·508	0 0 9·574
Ditto per Ton of Gross Load per Mile	0 0 0·166	0 0 0·216	0 0 0·165	0 0 0·135
Ditto ditto per Journey	0 1 6·459	0 2 0	0 1 6·37	0 1 2·94
COST OF OIL.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Total Cost	653 2 11	287 13 9	105 17 1	259 12 1
Cost per Engine per Journey (111 Miles)	0 3 0	0 2 11·520	0 2 9·977	0 3 3·427
Ditto per Mile	0 0 0·330	0 0 0·320	0 0 0·306	0 0 0·355
COST OF REPAIRS.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Total Cost per Engine and Tenders, as below	6991 16 4	4835 19 9	501 6 4	1654 10 3
Ditto per Journey (111 Miles)	1 12 8	2 9 9	0 13 5*	2 0 11
Ditto per Mile	0 0 3·539	0 0 5·380	0 0 1·450	0 0 2·264
Ditto per Ton of Gross Load per Mile	0 0 0·069	0 0 0·145	0 0 0·032	0 0 0·032
Ditto ditto per Journey	0 0 7·678	0 1 4·060	0 0 3·546	0 0 3·534

* *To account for this low figure* it must be remarked that only 9114 miles had been run by the engines previous to December 14, 1839.

LOCOMOTIVE POWER ACCOUNT,

From 15th December 1839 to 14th June 1840—(*continued*).

COST OF LOCOMOTIVE POWER.	Total.	DIVIDED ACCORDING TO ENGINES HAVING		
		Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Wheels, 5 ft. 6 in. Diameter; One Pair Driving Wheels.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Wheels, 5 ft. 6 in. Diameter; One Pair Driving Wheels.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Wheels, 5 ft. Diameter; Coupled Wheels.
	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Wages of Engine Drivers and Firemen	2961 9 10	1460 17 1	421 8 2	1079 4 7
Ditto per Mile travelled by the Engine	0 0 1·499			
Ditto per Journey of 111 Miles	0 13 10			
Ditto per Ton Gross Load per Mile	0 0 0·029			
Ditto ditto per Journey	0 0 3·252			
Coals and Firewood	813 11 2	370 3 6	142 7 10	300 19 10
Coke	16810 2 3	7216 2 9	2596 4 0	6997 15 6
Oil	653 2 11	287 13 9	105 17 1	259 12 1
Total Cost of Coals, Firewood, Coke, and Oil	18276 16 4			
Ditto per Mile travelled by the Engine	0 0 9·251			
Ditto per Journey of 111 Miles	4 5 7			
Ditto per Ton Gross Load per Mile	0 0 0·181			
Ditto ditto per Journey	0 1 8·072			
Pumping Engines	226 16 3	121 8 0	46 14 0	98 14 3
Hose Pipes	82 4 3	47 6 6	2 17 0	32 9 0
Fire Tools	8 0 4	4 6 10	0 8 0	3 5 6
Total Cost of Pumping Engines and Hose Pipes, and Fire Tools	357 0 10			
Ditto per Mile travelled by the Engine	0 0 0·181			
Ditto per Journey of 111 Miles	0 1 8			
Ditto per Ton Gross Load per Mile	0 0 0·004			
Ditto ditto per Journey	0 0 0·392			
Cleaners' Wages	3802 1 5	1729 19 8	665 8 10	1406 12 11
Waste and Oil for ditto	337 9 6	153 11 1	59 1 4	124 17 1
Total Cost of Cleaners' Wages, Waste, and Oil	4139 10 11			
Ditto per Mile travelled by the Engine	0 0 2·095			
Ditto per Journey of 111 Miles	0 19 5			
Ditto per Ton Gross Load per Mile	0 0 0·041			
Ditto ditto per Journey	0 0 4·546			
Repairs of Engines	6352 11 11	4447 8 5	443 1 4	1472 2 2
Ditto of Tenders	267 17 11	191 1 10	19 12 1	56 15 5
Cost of Files for Repairs	146 13 0	94 16 10	9 5 10	42 10 4
Ditto Painting ditto	224 13 6	102 4 8	39 6 6	83 2 4
Total Cost of Repairs for Engines, Tenders, Files, and Painting	6991 16 4			

LOCOMOTIVE POWER ACCOUNT,

From 15th December 1839 to 14th June 1840—(*continued*).

	Total.	DIVIDED ACCORDING TO ENGINES HAVING		
		Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Wheels, 5 ft. 6 in. Diameter; One Pair Driving Wheels.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Wheels, 5 ft. 6 in. Diameter: One Pair Driving Wheels.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in; Wheels, 5 ft. Diameter; Coupled Wheels.
COST OF LOCOMOTIVE POWER—(*continued*).	£. s. d.	£. s. d	£. s. d.	£. s. d.
Cost per Mile travelled by the Engine	0 0 3·539			
Ditto per Journey of 111 Miles	1 12 9			
Ditto per Ton Gross Load per Mile	0 0 0·069			
Ditto ditto per Journey	0 0 7·679			
Sundries	573 13 6	261 0 6	100 8 0	212 5 0
Ditto per Mile travelled by the Engine	0 0 0·291			
Ditto per Journey of 111 Miles	0 2 8			
Ditto per Ton Gross Load per Mile	0 0 0·006			
Ditto ditto per Journey	0 0 0·629			
Superintendence, Clerks, Foremen, Watchmen, Office Expenses, &c.	3189 2 0	1451 1 6	558 3 2	1179 17 4
Ditto per Mile travelled by the Engine	0 0 1·614			
Ditto per Journey of 111 Miles	0 14 11			
Ditto per Ton Gross Load per Mile	0 0 0·031			
Ditto ditto per Journey	0 0 3·502			
Total Cost of Passenger Trains	29562 5 7	17245 13 0	5185 2 11	7131 9 8
Ditto of Merchandize Trains	6927 4 2	693 17 11	15 0 10	6218 5 5
Ditto of Locomotive Power	36489 9 9	17939 10 11	5200 3 9	13349 15 1
Ditto per Mile travelled by the Engine	0 1 6·470	0 1 7·956	0 1 3·039	0 1 6·264
Ditto per Journey of 111 Miles	8 10 10	9 4 7*	6 19 1	8 8 11
Ditto per Ton Gross Load per Mile per Passenger Train	0 0 0·462	0 0 0·558	0 0 0·332	0 0 0·409
Ditto ditto ditto per Merchandize Train	0 0 0·187	0 0 0·276	0 0 0·253	0 0 0·180
Ditto ditto per Journey per Passenger Train	0 4 3	0 5 2	0 3 1	0 3 9·5
Ditto ditto ditto per Merchandize Train	0 1 8·75	0 2 6·5	0 2 4	0 1 8
Ditto per Ton Net Load per Mile per Passenger Train	0 0 2·263	0 0 2·731	0 0 1·624	0 0 2·005
Ditto ditto ditto per Merchandize Train	0 0 0·427	0 0 0·631	0 0 0·580	0 0 0·412
Ditto ditto per Journey per Passenger Train	1 0 11	1 5 3	0 15 0	0 18 6
Ditto ditto ditto per Merchandize Train	0 3 11·5	0 5 10	0 5 4·5	0 3 9·75
Ditto per Passenger per Mile	0 0 0·254	0 0 0·307	0 0 0·183	0 0 0·225
Ditto ditto per Journey of 111 Miles	0 2 4·25	0 2 10	0 1 8·5	0 2 1

* The excess of this amount is due to extensive repairs during this half-year.

EDWARD BURY.

LONDON AND BIRMINGHAM RAILWAY LOCOMOTIVE POWER ACCOUNT,

From 16th June to 15th December 1840, inclusive.

	Total.	DIVIDED ACCORDING TO ENGINES HAVING			
		Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels 5 ft. Diameter.	Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5ft. 6 in. Diameter.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. 6 in. Diameter.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. Diameter; Coupled Wheels.
		All Four-Wheeled Engines.			
Number of Locomotives	85	1*	42	12	30
Ditto Miles Run since the Opening of the Railway to December 15th, 1840 . .	2,190,493†	. .	1,325,310	196,194	668,989
Average Number of ditto by each Engine.	26,077	. .	31,555	16,350	22,300
Number of Miles Run from June 16 to December 15, 1840:—					
With Passenger Trains	452,660	2,421	252,164	103,514	94,561
With Merchandize Trains	105,102	244	9,936	579	94,343
Total	557,762	2,665	262,100	104,093	188,904
Number of Journeys (of 111 Miles):—					
With Passenger Trains	4,078	21¾	2,271¾	932½	852
With Merchandize Trains. . . .	947	2¼	89½	5¼	850
Total	5,025	24	2,361¼	937¾	1,702
Greatest Number of Miles Run during the same period by any one Engine . . .	. .	. .	13,410	12,122	9,181
Total Number of Passengers conveyed one Mile (distance 112 miles)	25,931,163				
Ditto (111 Miles) the distance performed by Locomotive Power	25,699,635				
Passengers apportioned among the several Engines, according to Gross Load . .	. .	158,582	13,146,250	6,623,437	5,771,366
Average Number of Passengers per Trip of Engine.	56¾	65½	52	64	61
WEIGHT CONVEYED.	Ton.				
Estimated Gross Weight of each Passenger with his luggage, including the portion of the weight of the Train due to each Passenger.	0·568				
Ditto Net Weight of ditto and Luggage (the portion of the weight of the Train due to each Passenger being excluded) 2¼ cwt.	0·1125				
The Net Weight of each Horse is averaged at 9 cwt.	0·22222				
Ditto Carriages with Baggage, 1 ton . .	1·				

* Harvey Combe (Ballast Engine). † Exclusive of Harvey Combe.

LOCOMOTIVE POWER ACCOUNT,

From 16th June to 15th December 1840, inclusive—(*continued*).

	Total.	DIVIDED ACCORDING TO ENGINES HAVING			
		Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. Diameter.	Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. 6 in. Diameter.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. 6 in. Diameter.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. Diameter; Coupled Wheels.
WEIGHT CONVEYED—(*continued*).	Tons.	Tons.	Tons.	Tons.	Tons.
The Net Weight of each Dog is averaged at 42 lbs.	0·01873				
Ditto Mail Bags, per diem, 4 tons, 8 cwt. 2 qrs.	4·425				
Ditto Cattle, 5½ cwt.	0·275				
Ditto Sheep, ¾ cwt.	0·0375				
Ditto Pigs, 1 cwt.	0·05				
Gross Load conveyed one Mile:—					
Passenger Trains	19,126,547	118,022	9,783,889	4,929,388	4,295,248
Merchandize Trains	10,075,712	20,328	711,212	45,048	9,299,124
Total	29,202,259	138,350	10,495,101	4,974,436	13,594,372
Net Load conveyed one Mile:—					
Passenger Trains	3,790,728	23,391	1,939,088	976,965	851,284
Merchandize Trains	4,912,183	9,911	346,735	21,962	4,533,575
Total	8,702,911	33,302	2,285,823	998,927	5,384,859
Average Gross Load per each Journey of the Engine (111 Miles):—					
Passenger Trains	42·254	48·749	38·800	47·621	45·423
Merchandize Trains	95·866	83·311	71·579	77·803	98·567
Average Net Load for each Journey of the Engine (111 Miles):—					
Passenger Trains	8·374	9·662	7·690	9·438	9·002
Merchandize Trains	46·737	40·619	34·897	37·931	48·054
Average Net Load to every 100 tons of Gross Load:—					
Passenger Trains	19·819				
Merchandize Trains	48·753				

LOCOMOTIVE POWER ACCOUNT,

From 16th June to 15th December 1840, inclusive—(*continued*).

	Total.	DIVIDED ACCORDING TO ENGINES HAVING			
		Cylinders 12 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. Diameter.	Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. 6 in. Diameter.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. 6 in. Diameter.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. Diameter; Coupled Wheels.
RATE OF SPEED.	Hours. Minutes.	Hours. Minutes.	Hours. Minutes.	Hours. Minutes.	Hours. Minutes.
Total Time the Passenger Trains were in Motion	17371 34	108 42	9770 31	3782 26	3709 55
Average Time per Journey (111 Miles)	4 15	4 59	4 18	4 3	4 21
	Miles per Hour.	Miles per Hour.	Miles per Hour.	Miles per Hour.	Miles per Hour.
Average Speed of Passenger Trains	26·080	22·271	25·808	27·367	25·489
Ditto Merchandize Trains	20·				
COKE CONSUMED.	Tons. Cwts.	Tons. Cwts.	Tons. Cwts.	Tons. Cwts.	Tons. Cwts.
Total Quantity	9844 6	52 13	4279 13	1611 18	3900 2
	lbs.	lbs.	lbs.	lbs.	lbs.
Quantity per Mile	39·535	44·254	36·575	34·687	46·247
	Tons. cwts. lbs.	Tons. cwts. qrs. lbs.	Tons. cwts. qrs.	Tons. cwts. qrs. lbs.	Tons. cwts. qrs. lbs.
Ditto per Journey	1 19 20	2 3 3 12	1 16 1	1 14 1 14	2 5 3 9
	lbs.	lbs.	lbs.	lbs.	lbs.
Ditto per Ton of Gross Load per Mile	0·755	0·853	0·913	0·726	0·643
Ditto ditto per Journey	83·818	94·621	101·389	80·568	71·333
Ditto per Hour	974½	975½	933¾	947¼	1036⅔
OIL CONSUMED.	Quarts.	Quarts.	Quarts.	Quarts.	Quarts.
Total Quantity	20050½	133½	9196	3242	7479
Quantity per Mile	0·036	0·050	0·035	0·031	0·040
Ditto per Journey	3·990	5·560	3·895	3·457	4·395
COST OF COKE.	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Total Cost	19420 13 10	103 9 5	8467 8 8	3150 5 3	7699 10 6
	d.	d.	d.	d.	d.
Cost per Mile	8·356	9·318	7·753	7·263	9·782
	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Ditto per Journey (111 Miles)	3 17 3	4 6 2	3 11 8	3 7 2	4 10 6
	s. d.	s. d.	s. d.	s. d.	s. d.
Ditto per Ton of Gross Load per Mile	0 0·160	0 0·179	0 0·194	0 0·152	0 0·136
Ditto ditto per Journey	1 5·760	1 7·869	1 9·534	1 4·872	1 3·096
Ditto per Hour	17 2	17 1 4/10	16 6	16 6 4/10	18 3¼
COST OF OIL.	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Total Cost	835 8 9	5 11 3	383 3 4	135 1 8	311 12 6
	s. d.	s. d.	s. d.	s. d.	s. d.
Cost per Mile	0 0·360	0 0·501	0 0·351	0 0·311	0 0·396
Ditto per Journey (111 Miles)	3 3·902	4 7·604	3 2·945	2 10·571	3 7·947

LOCOMOTIVE POWER ACCOUNT,

From 16th June to 15th December 1840, inclusive—*continued.*

	Total.	DIVIDED ACCORDING TO ENGINES HAVING			
		Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5ft. Diameter.	Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. 6 in. Diameter.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels. 5 ft. 6 in. Diameter.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. Diameter; Coupled Wheels
COST OF REPAIRS OF ENGINES AND TENDERS, INCLUDING PAINTING, &c.	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Total Cost	8247 1 2	73 12 8	4739 18 7	732 17 10	2700 12 1
Cost per Mile	d. 3·549	d. 6·631	d. 4·340	d. 1·690	d. 3·430
Ditto per Journey (111 Miles)	£. s. d. 1 12 10	£. s. d. 3 1 4	£. s. d. 2 0 2	s. d. 15 7½	£. s. d. 1 11 9
Cost per Ton of Gross Load per Mile	d. 0·068	d. 0·128	d. 0·109	d. 0·035	d. 0·048
Ditto ditto per Journey	7·523	14·178	12·031	3·925	5·291
COST OF LOCOMOTIVE POWER.	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Wages of Engine Drivers and Firemen	3470 10 7	16 11 8	1630 17 1	647 13 6	1175 8 4
Coals and Firewood	748 4 1	3 11 6	351 12 0	139 12 7	253 8 0
Coke	19420 13 10	103 9 5	8467 8 8	3150 5 3	7699 10 6
Oil	835 8 9	5 11 3	383 3 4	135 1 8	311 12 6
Pumping Engines at Stations, and Supply of Water at Birmingham	862 4 8	4 2 5	405 3 6	160 18 4	292 0 5
Hose Pipes	84 2 0	0 19 0	43 14 0	9 10 0	29 19 0
Fire Tools	11 5 4	0 8 0	5 6 4	1 10 4	4 0 8
Cleaners' and Labourers' Wages	4093 18 0	19 11 3	1923 15 8	764 1 0	1386 10 1
Waste and Oil for Ditto	393 7 2	1 17 7	184 16 11	73 8 2	133 4 6
Total	29919 14 5	156 2 1	13395 17 6	5082 0 10	11285 14 0
Repairs of Engines	7221 7 8	67 16 3	4335 3 10	590 10 2	2227 17 5
Ditto of Tenders	363 16 5	1 1 1	70 19 2	31 9 3	260 6 11
Cost of Files for Repairs	177 7 9	2 9 0	106 2 4	20 10 2	48 6 3
Ditto Painting	231 2 6	1 2 1	108 12 3	43 2 8	78 5 6
Cost of Working Stationary Engine at Wolverton (forming part of Repairs)	97 16 7	0 9 4	45 19 5	18 5 2	33 2 8
Repairs of Tender Wheels and Springs	155 10 3	0 14 11	73 1 7	29 0 5	52 13 4
Total	8247 1 2	73 12 8	4739 18 7	732 17 10	2700 12 1
Accident Account	139 19 7	0 13 4	65 15 7	26 2 6	47 8 2
Gas at Engine Houses, Camden, Wolverton, and Birmingham	317 4 7	1 10 4	149 1 6	59 4 0	107 8 9
Superintendent, Clerks, Office Charges, Foremen, &c.	4743 11 1	22 13 4	2229 1 11	885 5 0	1606 10 10
Total	5200 15 3	24 17 0	2443 19 0	970 11 6	1761 7 9
Aggregate of Totals	43367 10 10	254 11 9	20579 15 1	6785 10 2	15747 13 10

LOCOMOTIVE POWER ACCOUNT,

From 16th June to 15th December 1840, inclusive—(*continued*).

COST OF LOCOMOTIVE POWER—(*continued*).	Total Cost.	Cost per Day (183 Days).	Cost per Mile travelled by Engine.	Cost per Journey of 111 Miles.	Cost per Ton Gross Load per Mile.	Cost per Ton Gross Load per Journey.
	£ s. d.	£. s. d.	d.	£. s. d.	d.	d.
Wages of Engine Drivers and Firemen	3470 10 7	18 19 3½	1·494	0 13 10	0·028	3·166
Coals, Firewood, Coke, and Oil	21004 6 8	114 15 6½	9·037	4 3 7	0·172	19·161
Pumping Engines, Hose Pipes, and Fire Tools	957 12 0	5 4 8	0·414	0 3 10	0·008	0·874
Labourers and Cleaners, and Cotton Waste and Oil for Ditto	4487 5 2	24 10 5	1·929	0 17 10	0·037	4,094
	29919 14 5	163 9 11	12·874	5 19 1	0·245	27·295
Repairs of Engines and Tenders, including Files, Painting, &c.	8247 1 2	45 1 4	3·550	1 12 10	0·068	7·524
Sundries, viz., Accident Account, and Gas at Stations	457 4 2	2 9 11½	0·196	0 1 10	0·004	0·417
Superintendent, Clerks, Office Charges, Foremen, Watchmen, &c. &c.	4743 11 1	25 18 5	2·041	0 18 10	0·039	4·327
Total	43367 10 10	236 19 7½	18·661	8 12 7	0·356	39·563

	Total.	DIVIDED ACCORDING TO ENGINES HAVING			
		Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. Diameter.	Cylinders, 12 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. 6 in. Diameter.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. 6 in. Diameter.	Cylinders, 13 in. Diameter; Stroke of Piston, 18 in.; Driving Wheels, 5 ft. Diameter; Coupled Wheels.
	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Passenger Trains	34661 0 10	231 5 7	19815 7 10	6731 8 9	7882 18 8
Merchandize Trains	8706 10 0	23 6 2	780 15 8	37 13 0	7864 15 2
Total	43367 10 10	254 11 9	20596 3 6	6769 1 9	15747 13 10
	d.	d.	d.	d.	d.
Cost per Mile travelled by Engine	18·661	22·927	18·859	15·607	20·007
	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Ditto per Journey of 111 Miles	8 12 7	10 12 1	8 14 5	7 4 4	9 5 1
Cost per Ton of Gross Load per Mile:—					
Passenger Trains	0 0 0·435	0 0 0·470	0 0 0·486	0 0 0·328	0 0 0.440
Merchandize Trains	0 0 0·207	0 0 0·275	0 0 0·263	0 0 0·201	0 0 0·203
Cost per Ton of Gross Load per Journey:—					
Passenger Trains	0 4 0¼	0 4 4¼	0 4 6	0 3 0½	0 4 1
Merchandize Trains	0 1 11	0 2 6½	0 2 5¼	0 1 10¼	0 1 10½
Cost per Ton of Net Load per Mile:—					
Passenger Trains	0 0 2·194	0 0 2·373	0 0 2·452	0 0 1·654	0 0 2·222
Merchandize Trains	0 0 0·425	0 0 0·564	0 0 0·540	0 0 0·411	0 0 0·416
Cost per Ton of Net Load per Journey:—					
Passenger Trains	1 0 3½	1 1 11½	1 2 8¼	0 15 3½	1 0 6½
Merchandize Trains	0 3 11¼	0 5 2½	0 5 0	0 3 9½	0 3 10¼
Cost per Passenger per Mile	0 0 0·247	0 0 0·267	0 0 0·276	0 0 0·186	0 0 0·250
Ditto ditto per Journey	0 2 3¼	0 2 5½	0 2 6½	0 1 8½	0 2 3¾

LOCOMOTIVE POWER ACCOUNT.

REPAIRS OF ENGINES AND TENDERS,

From the Commencement of their Running to the 15th December 1840.

	Total.	DIVIDED ACCORDING TO THOSE THAT HAVE RUN				
		Under 10,000 Miles.	10,000 and under 20,000.	20,000 and under 30,000.	30,000 and under 40,000.	40,000 and under 50,000.
Number of Engines .	84	4	19	33	19	9
Total Miles Run . .	2,190,493	29,996	311,142	814,570	643,008	391,777
Average Number of Miles per Engine .	26,077	7,499	16,376	24,684	33,842	43,531
	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.	£. s. d.
Total Cost of Repairs	25037 1 6	137 0 3	2441 4 6	10163 10 4	7493 9 8	4801 16 9
Average Cost per Engine	298 1 2	34 5 1	128 9 9	307 19 9	394 7 11	533 10 9
	d.	d.	d.	d.	d.	d.
Ditto per Mile . .	2·743	1·096	1·833	2·994	2·797	2·941
	£. s. d.	s. d.	s. d.	£. s. d.	£. s. d.	£. s. d.
Ditto per Journey .	1 5 4½	10 1½	17 5	1 7 8½	1 5 10½	1 7 2½

EDWARD BURY.

XIII.—*An Account of the Mode of Construction adopted in Building a New Stone Bridge over the River Lea, at Stratford-le-Bow.*

By JOHN BALDRY REDMAN, Grad. Inst. C.E.

Read June 18th, 1839.

PREVIOUS to entering on a description of the present bridge, and of the works connected therewith, it may be well to give a short history of the ancient structure, which was one of the most interesting of the kind in this country; for, supposing any portion of it to have formed part of the original edifice, it must have possessed claims to antiquity to which few others could pretend, a bridge having been known to exist at this spot during the last seven centuries.

The Old Bridge.

Mr. Burges' History of the Bridge.

The following account is abstracted from a very able communication made by Mr. Alfred Burges to the Antiquarian Society in May, 1836, and published in the Archæologia, vol. xxvii.:—

After adverting to the celebrity of the bridge, the author thus proceeds,—"It is situated in the immediate neighbourhood of London, crossing the river Lea on the high road to Essex, at a distance not exceeding three miles from the metropolis.

"It was erected in the early part of the eleventh century, under the auspices of the pious Matilda, queen-consort of Henry I., to form a more direct and safe communication between the metropolis and the county of Essex than the then existing passage across the river by the dangerous ferry at the Old Ford."

Speaking of the general appearance of the bridge, Mr. Burges says,—"It possessed the character of building that marked the first attempts at bridge-building in this country: such as large piers formed for the support of small and low arched openings, and high battlements for the protection of a roadway of the narrowest possible dimensions."

The line of communication anterior to its erection is then described, and

Great Roman Road into Essex.

some references to the writers on Roman antiquities in this country are made, to justify the author's conclusion, that the Great Roman Road into Essex, crossed the river Lea by means of a ferry at Old Ford, in which direction it continued for many centuries after the Romans had left the island, until the erection of a bridge at Bow.

Early mention of the Road.

"This road is noticed as early as the seventh century, when the body of St. Erkenwald was stopped at Ilford and Stratford by the flood, as it was being conveyed from the abbey of Barking, where he died, for interment in London: this event occurred about A.D. 685, nearly 300 years after the Romans had left Britain."

Building of the First Bridge.

"Morant, in his history of Essex, has particularly noticed these roads, and the circumstances leading to the erection of the bridge, in the following passage:—'The ancient road from this county to London was by Old Ford, that is, through the ford then without a bridge; but that passage being difficult and dangerous, and many persons losing their lives, or being thoroughly wetted, which happened to be the case of Maud, queen consort of Henry I., she turned the road from Old Ford, to the place where it now is, between Stratford, Bow, and West Ham, and caused also the bridges and causeway to be built and made at her own charge.'"

The Bridge mentioned by Stow.

The accurate historian, Stow, also records this event in an interesting passage, which is in substance nearly the same as that already quoted: he further notices the grant of certain manors to the Abbess of Barking for the repairs of the bridge: these lands were afterwards purchased by Gilbert de Mountfichet, he agreeing to keep the bridge and road in repair; "till at length," says Stow, "hee laid the charge upon one Godfrey Prat, allowing him certain loaves of bread daily, that hee should repair the bridge and way; who being helpen by the ayde of travailers, did not only performe the charge, but was also a gainer to himselfe, which thing the abbot perceiving, hee withholdes from him parte of the bread promised: whereupon Godfrey demanded a toll of the way-faring men, and to them that denied he stopped the way, till at length, wearied with toyle, hee neglecteth his charge, whereof came the decay and ruine of the stone bridge and way."

Litigation relative to the Repairs of the Bridge, &c.

The memoir, after referring to other authors, gives some extracts from the records preserved in the Chapter-House at Westminster, furnishing detailed accounts of the proceedings in the Court of King's Bench, in the sixth and eighth years of Edward II., wherein it appears

that the Abbot of Stratford, the master of London Bridge, and the master of St. Thomas of Acre, were charged with the repairs, in consequence of their holding the lands originally granted by Matilda to the Abbess of Barking for the support and maintenance of the bridges and causeway.

In Easter Term, 9th Edward II., Godfrey, Abbot of Stratford, acknowledged his liability to keep them in repair as holding these lands, after many years of litigation in consequence of his having compelled persons to pay toll. "From this period," says the author, "to the dissolution of the monastery in the year 1539 (30 Henry VIII.), we do not find any attempt was made to throw off the responsibility: the bridges were, no doubt, during that period properly taken care of, and for some time after they fell into the hands of the Crown required but little repair, as we do not find any complaint being made until the year 1643, when they were again dilapidated."

At this period the holders of the lands originally granted for the repairs of the bridge denied their liability, upon the plea, that, the lands having gone to the Crown at the dissolution of the monastery, they were not therefore liable: a verdict was, however, given for the king. In 1663 the question was again agitated; but the landowners being advised by counsel, withdrew their opposition to the payment. In 1690 attempts were again made to throw off the burthen; but the decision was still in favour of the Crown, the Court being of opinion that all the lands of the former monastery were liable to the charge.

"From that period to the present day the landowners, profiting by the experience of the past, and not forgetting the wholesome advice of the honest lawyers of 1663, have continued the charge of the bridge and causeway at Stratford, for the free and uninterrupted use of the public, as was originally intended by the royal founder."

The great Essex Road.

The great Essex Road, traversing a rich agricultural district, and connecting some of the most influential towns and seaports of the south-eastern district of the kingdom with the metropolis, becomes necessarily a very important land communication; and to render it as perfect as possible several costly improvements have taken place from time to time: perhaps the most important of these alterations has been the reconstruction of Bow Bridge.

Derivation of the Name of the Bridge.

Authors have assigned to the word "Bow" various derivations. Stow says, "The bridge was arched like a bowe, a rare piece of worke, for before that the like had never been seen in England." And

again, he says, "Stratford Bridge being the first builded with arches of stone was therefore called Stratford-le-Bow." And Grose observes, that it might have derived its name from the word *beau*, an epithet very likely to have been given to it in those days.

The words "atte Boghe," or "atte Bowe," appear to have been added to the name of the village on the Middlesex side of the river, which is now known by the name of Stratford-le-Bow.

This venerable and time worn structure had, during the lapse of centuries, undergone many alterations; the piers and abutments were probably the only remaining portions of the original edifice; the pointed arches could have formed no part of the bridge of Queen Maud, as circular arches alone were used in those days; the pointed, or Tudor arches, were probably built subsequent to the 15th century.

Alteration in 1741.

The bridge was widened in 1741, by which the original elevation above the pointed arches was, in a great measure, obliterated: as that of the eastern arch remained more entire than the others, a separate elevation of it is given (Plate XIII.)

The piers occupied a large proportion of the waterway, and were placed at an angle to the stream, causing interruption to the current; but, unlike most of the old bridges in England, the piers were not surrounded by starlings: the shallowness of the water may account for this.

Description of Piers and Arches.

Very little attention appears to have been paid to uniformity, the piers being of different lengths and widths, and the arches springing from different levels. The side arches had a strong rib projecting below the soffit at the centre, as was at that period the general practice.

The elevation presented a very patched appearance from the numerous additions; and if any portion of the original structure remained, it was, as before stated, within the casing of the piers and abutments. The bridge was built of Portland and Kentish stone, with some Caen stone for the arches.

Character of the Masonry.

Mr. Burges concludes his account by saying,—"The filling in of the arches, between the face courses and the centre rib, is little better than rubble masonry, the stones of which are both rough and irregular in size, the joints wide, and in several places tiles are employed to wedge the whole together.

"The masonry of the centre arch is of a different character to that already described: the outside face courses are also in two thicknesses, composed of

Kentish ragstone with a few of Caen stone, which no doubt had been saved from a former arch; while the filling in between is entirely built of Kentish stone in regular courses, very neatly put together, and, as already stated, without any rib or other projection."

"The external face of the bridge above the arches is formed of common rubble masonry. The masonry of the additional arching, &c., made to the bridge in 1741, consists principally of Purbeck and Portland stone, built in regular courses, in a firm, substantial manner."

On referring to the drawing of the old bridge (Plate XIII.), it will be seen that the centre arch sprung from a higher level and was less pointed than the side ones, in order most probably to increase the waterway for the barges frequenting the navigation. From the projections in the sides of the piers, whence apparently the former arch sprung at a lower level, it would appear that the centre arch had been originally similar to the side ones: the elevation had likewise evidently been much altered in widening the bridge in 1741, by the addition of angular projections to the piers, and by extending the face of the bridge, on arches springing from these projections, at a higher level than the original ones. The extreme width of the bridge, before this alteration, was 18 feet 3 inches, including the side walls: by the above addition the clear width of the bridge was increased to 20 feet 5 inches at the west, and 22 feet 2 inches at the east end. The span of the side arches was 20 feet 3 inches, and that of the middle arch 22 feet 10 inches, making 63 feet 4 inches waterway; but the Middlesex arch was considered of little use for navigation, and its capabilities of drainage were much reduced by the lateral projection of the wharfs.

Dimensions of Old Bridge.

The soffits of the side arches in the centre were only 3 feet 9 inches above the level of high water at spring-tides, the middle arch being 2 feet higher; and an extraordinary flood has been known to close the side arches. From the above causes, and from the contraction of the waterway by the great thickness of the piers, and their direction not being in a line with the course of the river, the bridge was considered an obstruction to the free navigation, and also dangerous as a highway, on account of its narrowness and the want of footpaths: the wooden platform which had been erected on the north side affording only a partial accommodation to pedestrians.

The bridge being thus considered insufficient, Mr. Walker was requested, by the trustees of the Middlesex and Essex turnpike roads, to estimate the

expense of widening the bridge, and also to prepare plans and estimates for a new bridge of one arch. A report was accordingly made in January, 1828, which, after giving a short history of the old bridge, showed that, owing to the causes stated above, it occasioned a greater inconvenience to the navigation than even to the road-trust. Various estimates and plans accompanied this Report, that for a stone-bridge of one arch being the one selected. An Act of Parliament, for taking down the old bridge and erecting the new one, was obtained in 1834; and drawings of the bridge having been prepared by Messrs. Walker and Burges, the tender of Messrs. Curtis of 11,000*l.* founded on the same was accepted, and the preliminary operations were commenced in April, 1835.

New Bridge directed to be Built.

Demolition of the Old Bridge.

The temporary bridges being finished (as described page 349) and opened to the public in July, 1835, the removal of the old bridge was proceeded with, the best of the stones being preserved for the rubble masonry of the new bridge.

Old Roadway visible on Arch-stones.

The arches, during the removal, were supported by proper centering; when the covering was removed and the old roadway laid bare, the track of horses and cattle was clearly defined by a hollow worn into the arch stones, and on each side of it were deep wheel-cuts, on an average 6 to 8 inches deep and 9 inches wide, and distant from each other from centre to centre about 4 feet 6 inches, being worn in places to within 3 or 4 inches of the soffit: the Section (Plate XIII.) illustrates this more clearly. This most probably was the state of the bridge at one of those early periods already mentioned, when, owing to the disputes relative to the maintenance of the structure, it had become dilapidated. The piers were removed down to the bed of the river, and those portions of the abutments which interfered with the new work were taken down.

The New Bridge.

Dimensions of New Bridge.

The new bridge is constructed of Aberdeen granite: it consists of a single elliptical arch of 64 feet span on the square line and 66 feet on the skew, with a versed sine of 13 feet 9½ inches; the high water of ordinary spring tides rises to within 6 feet 9 inches of the crown of the arch, the low water level of the same tides being 7 feet lower. The foundations of the bridge are all laid upon the gravel, the clay substratum being 20 feet below the level of low water.

The width of the bridge between the parapets is 40 feet, consisting of a roadway 30 feet in width, and two paved footpaths of 5 feet each. The abutments on the square line are 42 feet $7\frac{3}{4}$ inches wide, and on the skew 43 feet $10\frac{1}{2}$ inches; their greatest thickness is 15 feet, with wing walls and counterforts each measuring 5 feet in thickness; the wing walls are 25 feet in length, and the counter-forts 15 feet, formed as shown (Plates XIV. and XV.) The total length of the bridge to the ends of the wing walls is 146 feet, the lengths of the retaining walls varying to suit the adjacent property.

Description of Arch.

The arch is composed of 65 voussoirs, varying from 4 feet to 2 feet 6 inches in depth, the latter being the dimensions of the key stone. The ashlar facing runs in horizontal courses, header and stretcher alternately, the former being from 2 feet to 3 feet long, and from 1 foot 3 inches to 1 foot 9 inches on the face; and the latter averaging 3 feet to 4 feet in length, by 1 foot 3 inches in depth: the heights of the courses vary from 22 inches to 12 inches, the upper courses increasing in height towards the centre, so as to range with the cornice.

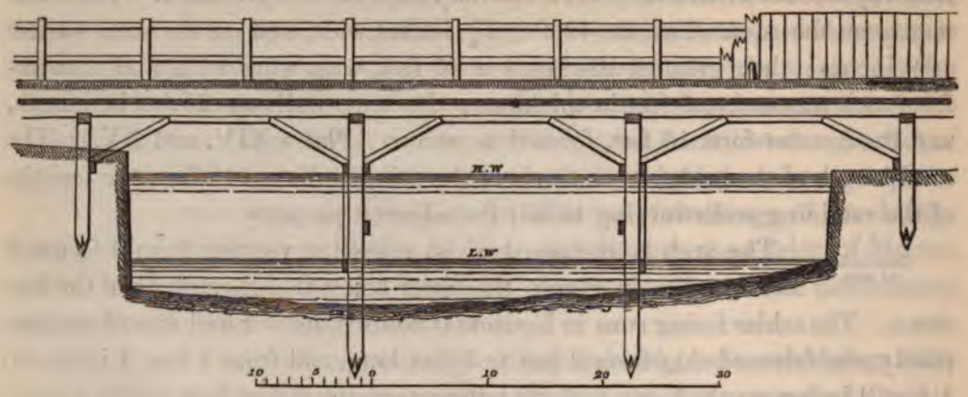
Temporary Bridges.

Before the old bridge was removed, it was necessary to construct temporary bridges, the situations of which are shown in the plan of the abutments and cofferdams (Plate XIV.) The temporary road bridge was supported on piles of whole timbers, with cap-sills 12 inches by 6 inches morticed on to them, and bearers 12 inches square above them to which the planking was spiked. The piles were driven 7 feet deep into the bed of the river; those within the lines of wharfing were from 7 feet to 9 feet below the level of the wharfs. The ends of the cap-sills projected beyond the line of the bridge and carried the posts of the railing, which were let through the planking and tenoned into them, being also secured by a wrought-iron strap round the post: wood spurs were likewise fixed to the outsides of each post, the planking projecting to receive the other end.

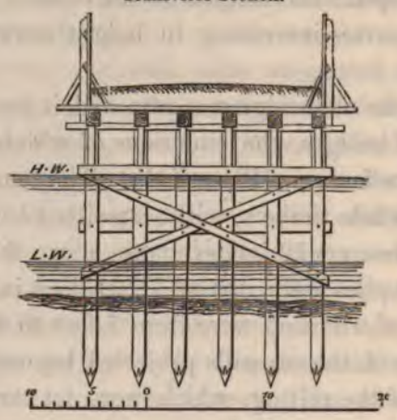
The piles were secured by braces and wales bolted to them. The fences on each side were 6 feet high above the roadway, consisting of planking 1 inch thick nailed to two rails in height, and a plank at the bottom for supporting the material of the roadway. Two recesses were formed in the fences on both sides of the bridge for the protection of foot passengers. The timber footbridge formerly added to the old bridge was removed to the north of the site of the new bridge, and being supported on suitable piles and bearers, formed a temporary bridge for pedestrians.

TEMPORARY BRIDGE.

Elevation.



Transverse Section.



Fender-Piles.

Before commencing any of the works connected with the new bridge which might by any means obstruct the navigation of the river, fender-piles of whole timbers, shod with iron, were driven 10 feet apart in front of each abutment, leaving a waterway of 26 feet.

Cofferdams.

The cofferdams for the erection of the new bridge were constructed in front of each abutment, formed to a regular curve, with a chord of 88 feet, and versed sine of 14 feet, abutting on each side against the adjacent wharfing, and projecting about 15 feet beyond the face of the abutments. The dams were formed of main piles 22 feet 6 inches long and 14 inches square, shod with iron and driven 9 feet below the bed of the river, at distances averaging 4 feet 3 inches from centre to centre.

The piles being driven, the waling-pieces were bolted to the outside; each wale consisted of two pieces of plank 13 inches wide by 3½ inches thick, making a waling 7 inches in thickness. A single row of whole timber sheet piles, each 22 feet 6 inches long with iron shoes, was then driven outside to a depth of 9 feet into the bed of the river. The sheet piles were each bolted to the walings, their tops levelled, and the whole well caulked, to render them perfectly water tight. A trench was excavated at the foot of the cofferdam on the outside, and filled with clay, and at the junction with the wharfs on the banks of the river the clay was protected from the current by planks laid on their edges, and supported by short piles; any leakage in the cofferdam was also more fully guarded against by driving piles inside the dams across the angles, with planking laid horizontally against them, and the whole of the intervening space filled in solidly with clay: this piling and planking was further secured by being bolted to the dam.

As the excavation for the abutments proceeded, the cofferdams were further strengthened by part of the excavated material being thrown against the foot of the sheet piling on the inner side; struts, about 45 feet in length and 12 inches square, were placed with one end resting on the ground at the back of the abutments, and the other placed upon cleats against the main piles.

Each dam was furnished with a trunk to which a water-tight valve was fitted for letting off the water; a well was also formed in front of the abutments for the collection of the water, which flowed freely from the land-springs on the Essex side, so that a double-headed pump, worked by twelve men, was kept fully at work draining the cofferdam: on the Middlesex side there was less water.

Foundations.

The dams being closed, the excavation was carried to a depth of 12 inches below the lowest course of footings; the area of the foundations was enclosed by iron-shod sheet piling of elm timber, 10 feet long and 6 inches thick, sawn on the front, planed straight on the edges, and driven vertically into the ground. Great care was taken to drive these piles perfectly straight and close, so as to form, when the tops were levelled, a solid and compact foundation. A wale of sawn timber, 12 inches by 6 inches, was secured outside and bolted to each pile: the whole was secured to the foundations at distances of 6 feet apart by wrought-iron ties, 1½ inch diameter, with jagged ends, let into the upper face of the first course of footings: they were run with lead at one end, and screwed up outside against cast-iron washers let into the waling.

Laying the First Stone.

The first stone (containing a plate bearing an inscription, with the names of the Committee of Trustees, &c., and some coins of the realm) was laid on the 12th December, 1835.

Concrete.

The concrete was composed of one measure of fresh burnt ground lime, to eight measures of river gravel and sand, mixed dry; water was then added, and the whole mixed together, and in this state thrown in from a height of about 10 feet.

Footings.

A bed of this concrete, 12 inches thick, was placed on the site of the foundations, and upon this the lower course of footings was laid: this and the two next courses of footings were formed of Roche Portland stone. The beds and joints were worked square and levelled, and the face and upper surface of the offsets picked fair; the whole was laid in puzzolana mortar, carefully mixed in the proportion of one measure and a half of sand, one of puzzolana, and one of lime; the lime was slaked and mixed with the sand, passed through screens, and afterwards worked to a proper consistency by a horse-mill; a grouting composed of the same materials was poured into the vertical joints. The arch-stones were also completely embedded in the same material.

Puzzolana.

Mortar.

The mortar for the interior stone-work, the sides of the abutments, and the upper work, was composed of three measures of sand to one of lime.

Rubble backing.

As the courses of the abutments advanced, the backing was brought regularly up to the same level, the best stones from the old bridge being taken for this purpose; the rubble stones composed the mass, and the largest and squarest pieces laid alternately header and stretcher formed the face. The rubble throughout was laid carefully by hand, rammed down, and flushed up solid in mortar. When the stone from the old bridge failed, Kentish ragstone and brickwork were introduced. The best of the excavated material was rammed in front of the wing walls; and at the back, the whole was filled in solid with concrete to the slopes of the excavation.

Centering.

After four courses of granite were completed, the arch-stones set at the springings, and the backing advanced to the same level, the first truss of the centering was set up at each side, upon the supporting piles; the work was then advanced to the sixth course upon the first trusses, when the dams were removed: four more courses were then added on the Essex side and two on the Middlesex side, after which the centering was completed: it consisted of eight ribs framed, as shown in Plate XV., bound at the joints by

wrought iron straps and keys, and supported on piles 15 feet long, driven 7 feet into the bed of the river. The timber used was Dantzig fir, with the exception of the king-posts and striking wedges, which were of English oak.

The scantlings of the various parts were as follows, viz.:—

Piles	12 inches	×	12 inches
Sills	12 „	×	6 „
Principal framing .	12 „	×	12 „
Struts	10 „	×	10 „
Braces	10 „	×	5 „
Kings	12 „	×	14 „

After the centering was framed, the timbers being properly bolted together, and the tressels and cap-sills strongly braced, each rib was accurately shaped to a curve 6 inches below the soffit of the arch, by blocking pieces fixed to the ribs, of the same width, and carefully adjusted to the requisite curve; the covering of fir planking 6 inches thick for carrying the arch stones was then laid and spiked to the ribs. The centering being completed, the work proceeded; the crown of the centering being loaded with the upper arch-stones, to prevent it from yielding to the pressure caused by the laying and setting of the lower arch-stones.

Ashlar Masonry.

The north side of the Middlesex abutment next the washway, from the footings upwards as well as the wing-walls, are faced completely with granite ashlar. The courses of ashlar were flushed up solid in mortar, the stones laid header and stretcher alternately; none of the joints exceeded an eighth of an inch in thickness, the front ones being pointed with cement: care was taken that, when set, the stones should lie on their natural beds, nor was any packing whatever allowed. The ashlar was faced with a draught round the outside, and the space within fine-picked, no stone breaking-joint on the face less than 12 inches.

The Arch.

To form the curve of the arch, and the templates for the arch-stones, the chord and versed sine were laid out on a floor, and the ordinates to the lower and upper angles of the arch-stones calculated and set off; moulds or templates were formed from this diagram, and the stones were worked from them; the moulds for the centering were likewise formed from this curve.

The arch-stones are set parallel to the abutments, consequently the construction is the same as though the arch had been square, the skew or angle of the arch being worked out on the face.

The arch-stones, after being carefully set, were driven close together, and the key stones in closing the arch fitted extremely tight and solidly. The last key stone, containing, in a recess cut in its bed, a bronze medal of Her Majesty Queen Victoria, was laid on the 31st of January, 1838, by John Henry Pelly, Esq.,* the chairman of the Trustees, to whose indefatigable zeal the public are principally indebted for this useful work.

Striking of the Centering.

The arch being keyed and time having been allowed for its settling, the wedges of the centering were eased on the 13th March, 1838, and the next day the centering was completely struck; the level of the arch was tried on the 15th of the same month, both at the north and south side, and it was found to have subsided half an inch from the first easing of the centering; during the erection the structure had settled one inch; in setting out the work 2 inches had been allowed for subsidence, the rise of the arch being made 13 feet 11 inches, for 13 feet 9 inches, the intended rise leaving still half an inch for any future settlement, of which, however, there is not at present any appearance.

Cornice, Parapets, &c.

The arch being completed, and the masonry of the abutments and wing-walls brought up to the proper level, the cornice course was set, the stones averaging 4 feet in length, attached together on the upper bed by joggles of Yorkshire stone 4 inches square by 3 inches long, let flush in. (Plate XV.)

The inclination of the cornice on each side from the centre of the bridge to the finishing pedestals is 12 inches, which is the same as that of the footpaths.

The cornice courses being set, the parapets, which, including the plinth, are in single stones, were next constructed: their height is 5 feet 3 inches, by 12 inches thick, with a projection of 2 inches outside the base, 18 inches in height, forming an outer plinth; the inner face being flush from the foot pavement upwards, thereby increasing the width of the foot ways.

The parapet stones average from 3 to 4 feet in length: they are secured at the joints and at bottom, to the cornice, by joggles 6 inches long, 4 inches square in the middle, and tapering at the ends: each stone was set up in its place by means of an iron clamp of a horse-shoe shape, which, embracing the upper part of the stone and being secured to it by a screw, enabled it to be lifted to its position by a block tackle. The face of the stone was protected from injury by a piece of lead placed beneath the point of the screw.

* Now Sir John Henry Pelly, Bart.

By this means the stones were accurately bedded before any mortar was used; any inequality in the bed or otherwise was rectified, and they were then set in puzzolana mortar: a channel having been cut in the joints of the stones from the top to the bottom, leading to the joggle holes, liquid grouting was poured in until it filled up all the interstices. The pedestals at the ends of the wing-walls were set and joggled in like manner to the parapet.

Concrete Foundation for the Roadway.

The crown of the arch and tops of the abutments were covered with concrete 1 foot 6 inches thick, composed of the same materials as that used for the foundations; on this bed were laid the foot-pavements of Aberdeen granite, in stones of 4 feet 4 inches long, and 6 inches thick, averaging 2 feet to 3 feet in breadth, bounded by curb stones 8 inches wide, outside of which three rows of pitching-stones were placed.

The Roadway.

The roadway was formed of screened gravel, to the depth of 6 inches over the crown of the arch, bedding immediately upon the concrete substratum, and to a like depth over the abutments. It ranges with the cornice in its longitudinal direction, and falls 3 inches from the centre to the curb stone on each side.

The Approaches.

The approaches were formed of a bed of gravel, over which a 6-inch dressing of screened gravel was laid, the whole being graduated to an inclination of one in thirty. On the Middlesex side, north and south, and on the Essex side to the south, brick retaining walls capped with granite were built, and the footpaths of the approaches were protected by granite curbs. The bridge, being completed, was opened to the public on the 14th February, 1839.

The engineers intended this bridge to have been entirely constructed of Aberdeen granite, but the difficulty experienced in procuring a sufficient supply, without delaying the work, rendered it advisable to permit the contractors to construct the north parapet of Penryn granite, and to substitute Herm and Penryn stone for three of the arch-stones; it is nevertheless perhaps the most complete specimen of masonry, in Aberdeen granite, in the neighbourhood of London.

JOHN B. REDMAN.

Limehouse, May, 1839.

XIV.—*Observations on the Effects produced by Wind on the Suspension Bridge over the Menai Strait, more especially as relates to the Injuries sustained by the Roadways during the Storm of January,* 1839; *together with brief Notices of various Suggestions for Repairing the Structure.*

By W. A. PROVIS, M. Inst. C.E.

Read February 16, 1841.

MANY erroneous and contradictory reports having been circulated as to the injurious effects of winds on the Menai Bridge, I have thought it right to record such of the principal facts relating to the matter as have come within my knowledge; and believing that they will not be uninteresting, I am induced to submit them to the consideration of the Institution of Civil Engineers.

Movement of the Chains.

In the months of December, 1825, and January, 1826, when the bridge was nearly completed, several severe gales occurred in succession, and considerable motion was observed both in the main chains and the platform of the carriage ways and footpath. At that time the four lines of chains were not transversely connected together, nor were the roadways finished; but as it appeared that the chains were not always acted upon by the wind simultaneously, nor with equal intensity, it was believed that by attaching them to each other, so as to retain them in parallel planes, and thus check the tendency to motion of any one of the lines by the *vis inertiæ* of those which were not similarly influenced, the total amount of movement would be diminished or neutralized.

First Injury by Wind.

Though the parapet railing and some unimportant parts had not been then completed, the bridge was opened to the public on the 30th January, 1826. On the 6th February following there was a heavy gale, which blew some of the yet unfixed parapet into the strait, and caused so much motion in the chains and roadways, as to break several of the vertical suspending rods, and many of the iron bearers or joists which supported the platform of the roadways.

Construction of Bearers.

The bearers or joists were each composed of four horizontal bars, two of them 12 feet long, and the other two 16 feet long each.

The bridge being 28 feet wide, a bar of 12 feet long, with one of 16 feet, made up the total width; and these two being connected with the other two similar bars, having that of 12 feet long placed alongside that previously laid of 16 feet, and the second 16 feet bar alongside the first of 12 feet, the two longer bars thereby overlapped each other 4 feet (or the width of the footpath) in the middle of the bridge. The eyes at the lower ends of the four vertical suspending rods, which ranged in the transverse direction of the bridge at the respective distances of 12, 4 and 12 feet from each other, were passed between the two pairs of horizontal bars and bolted through them. Beneath each of the 12 feet lengths a tie-rod with a king-post was introduced, to prevent the bars from bending between their points of suspension. All these were screwed together so as to become in effect one nearly inflexible joist suspended at four different points of its length.

Undulation.

The motion which had been anticipated was that of simple undulation flowing in waves at right angles to the length of the bridge; and had this been the only motion, the bearers, it is apprehended, would not have been injured by the gale. They would have been equally raised or depressed throughout their lengths as the wave rolled forward, and been subjected to no strain which they were not fully competent to resist.

The movement of this undulatory wave, however, was oblique with the lines of the bearers and their suspending rods as well as with the general direction of the bridge. In other words, when the summit of the wave was at a given point on the windward side of the bridge, it was not opposite this point on the leeward side, but, in relation to the flow of the wave, considerably behind it; the motion appearing to be generated on the windward side, and by the time it had crossed to the leeward it had also moved forward along that on which it had commenced.

Consequences of Undulation.

The tendency of this undulation was to bend every bearer into a form produced by the oblique intersection of a vertical plane with the surface of the moving wave, and this form necessarily varied with every change in the position of the wave.

To this cause must be attributed the failure of those bearers which were broken by the gale. They had been constructed for the purpose of resisting the species of strain to which it was expected they would be exposed, but they were deficient in strength where that strain was greatest. They were uniformly fractured at the eyes of the 16 feet bars where they were suspended from

the rods on each side of the footpath, and at those parts of the bridge which were about midway between its centre and the supporting pyramids.

Amount of Motion.

The motion was observed to be greatest about half way between the pyramids and the centre of the bridge. The wave increased in its progress from the pyramid till it attained its maximum altitude near the first quarter, and at the same instant the extreme depression was near the third quarter. The wave then gradually diminished to the centre of the bridge and afterwards increased to the third quarter, when it attained its greatest height at the same time that the first quarter was most depressed. The platform and the main chains were equally subjected to this undulatory motion: the latter also oscillated slightly.

The Platform.

The bearers or joists supported the double tier of planks which constituted the principal part of the platform. Both tiers were placed longitudinally, as giving most strength for supporting passing loads. They were considered to be sufficiently connected transversely by the iron joists upon which they rested.

Cause of the failure of Suspending rods.

The planks forming the roadways were bolted to the bearers, and notched to fit closely around the lower ends of the vertical suspending rods, which were thereby held almost immovably in the platform.

During the undulations to which the bridge was exposed, the surface of the roadway continually changed its position relatively to the lines of the suspending rods; the latter preserving their tendency to hang vertically as when in a state of rest, whilst the platform changed its relative angle with every variation of the moving wave. This bent the suspending rods backwards and forwards at the parts where they were held fast at the surface of the roadways, and there, in many instances, wrenched them asunder.

The failure of these suspending rods and bearers was evidently to be attributed to the character as much as to the amount of motion which had existed during the gale. It was also obvious that, owing to the peculiar nature of this motion not having been anticipated, some of the means which had been adopted to give rigidity to the platform had not possessed sufficient strength, whilst others had acted prejudicially.

Such were the principal injuries caused by the gale; and as the bridge had been opened a week for public intercourse, it was necessary to apply immediate remedies.

Repairs.

There were a few spare suspending rods on the spot, which were immediately used to replace those which had been broken

or damaged. The planks of the platform were cut away for about an inch round all the suspending rods, so as to give them room to play where they had previously been held firmly.

Beneath each place of fracture of the roadway bearers an iron stirrup was passed, having a broad sole at the bottom to support the broken ends of the horizontal bars, and a pair of eyes at the upper extremities by which it was suspended from the vertical rod.

Stiffening Planks.

Between each two bearers an oak plank, about 15 feet long, was introduced and bolted to the underside of the platform, thereby tying it together in its transverse direction, and giving it a degree of stiffness combined with elasticity which it had not previously possessed.

Main chains connected.

The four lines of main chains were also at the same time connected by placing cast iron tubes horizontally and transversely between them, and tying the whole together by wrought iron bolts passed through the tubes and the joint plates of the chains.

When the repairs were completed, the beneficial results of the recent additions were evident. Storms occasionally broke a suspending rod or a roadway bar, but these circumstances rarely occurred, and the injuries were repaired without difficulty or delay.

Storm of 1836.

On the 23rd January, 1836, (ten years after the bridge was first opened to the public,) an unusually severe gale of wind from the W. S. W. caused the chains and platform to undulate violently, and broke some of the roadway bearers with six of the suspending rods. There can be no doubt that ten years' continued friction, combined with the ordinary shrinking of the timber, had considerably affected the original rigidity of the platform, so that during the gale last referred to there was a greater degree of irregular undulation than had previously been observed. It is not easy to define correctly the amount of rise and fall of the roadways, but Henry Fisher, the bridge keeper, who assisted as a carpenter in the original construction, had lived ever since at the Anglesea end of the bridge, and watched every storm by which it had been assailed, stated that there was little less than 16 feet between the extreme of rise and the lowest point of depression.

Oscillation of the Chains.

There was also considerable oscillation of the main chains between the pyramids and the centre of the bridge—not to an extent that could have produced a great proportion of the observed rise and fall of the roadways, though there is no doubt that it was one of the aiding causes.

Platform not fixed at the ends.

The extremities of the suspended part of the platform were not attached to the two pyramids, but rested upon the masonry, the wheel guides and part of the planking being constructed so as to slide under the arches of the carriage ways, in order to prevent the platform being easily blown out of its place. The gale of January 23, however, broke the wheel guides and planks at the Anglesea pyramid, and forced that end of the platform sideways about 15 inches from its true position. By cutting away the ends of the planks which were jammed against the masonry of the pyramid, and then applying some powerful hauling tackle, the platform was restored to its proper situation.

Provis and Rhodes' Survey.

In consequence of the injurious effects of this gale, and likewise of various statements which had been circulated respecting the bridge, the Commissioners of Her Majesty's Woods directed me, in conjunction with Mr. Rhodes (who had originally superintended the fixing of the iron and timber work of the bridge), to examine the whole structure very carefully, and to report to the Board our observations on its state, as well as on any repairs or additional works which might appear necessary for increasing its strength or security.

Report upon the Bridge.

The result of our examination was satisfactory: the whole of the masonry, the main chains, their fastenings to the rock, the rollers and other iron work on the supporting pyramids, and all the principal parts of the bridge, were as perfect as when first constructed. Wherever a suspending rod had been broken it had been replaced by a new one, and the stirrups which had been applied to carry the damaged bearers of the platform had answered the intended purpose.

We were well aware that the suspended portion of the bridge possessed an amount of flexibility, as well as a susceptibility of motion which it was very desirable should be reduced: we therefore stated it to be our concurrent opinion, "that a greater degree of rigidity should be given to the roadways, so that they should not bend so easily under vertical pressure."

The bridge, however, remained in the state which has been described, from the period when our examination was made, until the memorable hurricane of the 6th and 7th January, 1839.

Storm of January, 1839.

The storm, according to the account of those who observed it at the bridge, commenced on the evening of the 6th with a strong but unsteady wind veering a few points on both sides of S. W. The gale increased during the night so much, that between 2 and 4 o'clock of the following morning it became a hurricane, and all approach to the suspended part of the bridge was

impracticable. The fury of the storm having somewhat abated after 4 o'clock (A.M.), the bridge keeper made an attempt to examine the chains and roadways. It was with difficulty he could make his way to the platform, and it was only by watching the lulls of the wind and holding fast by the iron-work that he was enabled to reach the suspended part of the bridge. The general darkness was too great and the tempest too violent to allow him to make any close or accurate observation, but he ascertained during occasional gleams of moonlight that the roadways were broken through, and that it was necessary to take immediate measures for preventing any attempt to cross the bridge.

He returned and closed the approach at the Anglesea end; then procuring a boat, he reached the opposite side of the Strait just before the arrival of the down mail from London. The guard and bags were conveyed across the Strait in the boat, and information of what had happened was forwarded to Mr. John Provis at Holyhead, under whose care the bridge had been placed after it was first opened to the public.

Effects of the Storm.

When daylight arrived the general injury was evident. Both carriage-ways had been partially destroyed and were rendered impassable; the footpath between the carriage-ways however was but slightly injured, and the bridge could still be traversed on this central line.

The weather continued too tempestuous to admit of men working on the bridge, and the day was spent in making preparations for a temporary repair of the carriage-ways as soon as the wind should subside.

Details of the Injuries.

The plan and elevation given in Plate XVI. explain the nature of the injuries sustained by the bridge: they exhibit, as well as the small scale of the drawings will permit, the several fractures of the roadways and the general appearance of the suspended part immediately after the storm. The elevation is represented as it would appear to a person looking to the north east or towards Beaumaris.

The condition of the two carriage-ways was as follows:—

1st. *The North East Carriage-way.*

a. to *b.*, length 80 feet.—Very slightly injured.
b. to *c.*, ,, 55 feet.—One-half of the outer suspending rods broken.
c. to *d.*, ,, 15 feet.—The suspending rods on both sides torn asunder, the roadway broken through at the line *d*, and the end of the platform at *e* hanging down about 6 feet.

d. to *f.*, length 175 feet.—The whole of the suspending rods on the inner side broken, and with the exception of 6 rods near the N.W. end and 7 rods near the S.E. end, all those on the outer side were also broken, the timber platform hanging down suspended by the rods at its two corners.

f. to *g.*, „ 85 feet.—All the suspending rods on the outer side broken, on the inner side all broken except 4 near the N.W. end, the north corner of the platform hanging perpendicularly down.

g. to *h.*, „ 55 feet.—All the suspending rods on the inner side broken, and the outer side suspended only by 4 rods.

h. to *i.*, „ 85 feet.—Not injured.

2ndly. *The South West Carriage-way.*

k. to *l.*, length 123 feet.—Not damaged.

l. to *m.*, „ 40 feet.—All the outer suspending rods broken, the platform broken across at *m*, and the corner *n* hanging down about 6 feet.

m. to *o.*, „ 267 feet.—Scarcely injured.

o. to *p.*, „ 30 feet.—All the outer suspending rods broken, and the timber platform at that edge sunk about 6 inches in the middle of its length.

p. to *q.*, „ 80 feet.—Not injured.

Number of Rods broken.

From the foregoing statement it appears that out of a total number of 444 suspending rods rather more than one third had been torn asunder, that the timber work of the S.W. carriage-way had been broken through transversely at one place, and that of the N.E. carriage-way in two places; and that of the latter there was one connected piece of 175 feet in length and 12 feet in width hanging down, and swinging in the air.

Iron Joists broken.

In those parts where the suspending rods had been torn asunder and the platform broken down, the iron joists or bearers had also failed; and much of the parapet railing had been blown into the Strait.

The Ties, &c., destroyed.

The ties and tubes before described, by which the main chains were kept in parallel planes, were destroyed.

Main-chains.

The main chains had sustained the shock admirably; three only of the common bars and one of the adjusting bars having been damaged. These were between the two supporting pyramids, and taking into calculation the total number of bars betwixt those masses of masonry, the proportion they bore to the whole was less than one in a thousand. The injured bars were cracked at the eyes.

Screw bolts.

Two or three of the screw bolts which connected the bars of the main chains into links had their heads struck off;—a fact which may give some idea of the tremendous violence of the storm: for it was quite clear that though the chains were suspended in parallel lines (those on either side of each carriage-way being 12 feet apart) they had, after the breaking of the transverse ties and tubes, been thrown so violently against each other as to cause deep indentations in the iron and to break off the heads of the bolts, the shanks of which were 3 inches in diameter.

Movement of the Platform.

It was also evident from distinct marks on the masonry of the Anglesea pyramid, that after the fracture of the wheel guides, &c., which passed under the roadway arches, the platform had oscillated considerably in its transverse direction, the marks extending at least 3 feet on the leeward side of the platform.

Carriage-way restored.

The S. W. carriage-way having been least injured it was determined on the morning of the 8th to employ all the workmen on that side, so as to render it passable with the least possible delay. The suspending rods which had been broken, were replaced by new ones as far as the spare stock at the bridge would permit, and when these were exhausted chains and ropes were substituted. That portion of the timber platform which had been broken down was lifted again into its place by means of pulley blocks, suspended from the main chains and attached to the most depressed corner of the platform. The broken platform was thus raised to its original position, where it was temporarily fastened by means which have been already described.

Pending these operations I had been apprised of the injuries which the bridge had sustained, and had been directed by the Commissioners of Her Majesty's Woods and Forests to examine the bridge very carefully, and report to the Board my observations upon the injuries sustained, and to suggest the best method of repairing them.

Passage restored.

On arriving at the bridge on the 11th January I found that, through the exertions of Mr. John Provis and Henry Fisher

(the resident engineer and the bridge keeper), the S.W. carriage-way had been so far restored, by the means already described, as to allow the mail to pass over the bridge as usual. From that time the public had the uninterrupted use of one carriage-way until the 21st January, when the temporary repair of the N.E. side of the platform having also been completed, the whole of the bridge was again opened for general use.

It has thus been shown that though the damage done to the suspending rods and platform was considerable, foot passengers were not prevented from crossing the bridge at any time, and carriages and horses for only five days.

Proposed Repairs.

In the Report which I submitted to the Commissioners of Woods and Forests it was proposed to effect the following alterations in the construction of the roadways, which were suggested by the experience of thirteen years' observation.

First.—That the new bearers or joists of the platform should be composed of three several lengths, with joints at each point of suspension so as to permit of an action in the transverse direction of the bridge without incurring the risk of the joists being broken, as had occurred with the original inflexible bearers.

Secondly.—To counteract the flexibility so imparted by introducing a complete course of transverse planking between the two courses of longitudinal planks, thus giving a greater degree of transverse rigidity than before with a material possessing more elasticity than iron, and consequently not so liable to fracture.

Thirdly.—To introduce a joint in the suspending rods at the upper surface of the platform, at right angles to that which connected the lower ends of the rods with the joists, so as to prevent their being broken.

Fourthly.—To render the whole platform more rigid in its longitudinal direction by placing two lines of beams along the upper and lower surfaces, bolted through each other and the three tiers of planks.

Fifthly.—To renew the transverse ties and hollow distance pieces which connected together the four lines of main chains.

Restoration ordered.

As other modes of restoration had been suggested, and were subsequently proposed both to the Lords of the Treasury and the Commissioners of Her Majesty's Woods and Forests, some time was necessarily occupied in giving to the subject that mature consideration to which the various schemes were entitled. I was ultimately directed to carry into execution the

renewal of the roadways and other parts of the bridge in conformity with my Report and plans.

The works have since been satisfactorily completed, and as a detailed account of the difference in the construction of the old and new platform, with reasons for the several deviations from the original plan, are presented to the Institution by Mr. T. J. Maude, the resident engineer charged with the superintendence of the late restorations, his communication* may be referred to for further particulars respecting this part of the subject.

Other Plans for the Repairs.

There now only remains to describe briefly some of the other plans which were submitted at that period for restoring and securing the roadways.

Mr. Comins' Plan.

Mr. N. P. Comins proposed—"to connect the middle of the bridge by means of chains with the pier on either side midway between the water and the bridge, and to fix chains parallel with these till they approached the apices of the angles formed by the bridge and the piers. The chains should be connected laterally and with perpendicular bars as are the chains above the bridge, to which they should in every respect be similar."

The objections to this gentleman's plan were that his chains, &c., would interfere most injuriously with the free navigation of the Strait, which it is as important to maintain as to preserve a safe passage over the bridge; and that such interference would be a departure from the pledge given to the public when the Bill for sanctioning the construction of the bridge was before Parliament, as well as a breach of the provisions of the Act which was subsequently passed.

Henry Fisher's Plan.

Another mode of tying down the bridge was suggested by Henry Fisher. His plan was to fix thirteen oak beams transversely beneath the platform at unequal distances, diminishing from the pyramids towards the centre of the bridge. Then from the masonry of either pyramid and on a level with the roadway he proposed to carry a pair of iron rods diagonally to a point of the main chains perpendicularly over the first oak beam, to return a pair of rods from the same point of the main chains down to the second oak beam; then carry others up to the chains over the third and down again to the fourth beams, and so on over the whole front of the bridge. He then proposed to cross these rods by others commencing at the top of the pyramid and passing

* See page 371.

down to the first beam, again up to the chains and down to the third beam, and so on throughout the length of the platform. By proceeding thus at each of the four lines of main chains he obtained four series of reticulated ties or braces between them and the platform, connected at both ends of the suspended part of the bridge with the solid masonry of the pyramids. There could be no doubt that this plan would have reduced the motion of the main chains and platform, but it appeared to me that it would not entirely prevent it, and would involve, in the diminished movement, a complexity of action which it was very desirable to avoid. Besides the diagonal braces would have spoiled the general symmetry of the bridge, and have caused such an amount of friction against the vertical suspending rods as might ultimately have cut them through.

Tying the Platform from below.

In reference to tying down the platform from below, it was observed by Mr. John Scott Russell that the points at which the ties should be attached to the platform should be determined by those laws which govern the vibrations of an elastic cord in a state of tension. Those laws might apply if the whole suspended part of the bridge acted simply as one string; but it is composed of many, of different materials, sizes, and degrees of tension, bound together in a variety of ways. It therefore seems doubtful (though the subject is well worthy of investigation) whether the principle admits of correct application under the complicated combinations of a suspension bridge.

Colonel Pasley's opinion.

In the communication which was made to the Institution by Colonel Pasley,* as well as in that which he laid before the Commissioners of Her Majesty's Woods, it has been assumed—"that all the injuries that have ever occurred to the roadways of suspension bridges must have been caused by the violent action of the wind from below." In this opinion I do not fully concur, and I shall state some of the reasons which induce me to think that the conclusion is erroneous.

The action of Wind.

Supposing a current of wind were to rush down from a high ridge of ground to the surface of a river or valley, it would rebound from the place on which it first impinged, but with a force less than that of the original descending current. If a suspension bridge were placed across the valley beyond the spot where the current first struck its surface, but still within the influence of the rebound, the platform of the bridge would be acted upon most powerfully from below. But if the bridge were situated in the direct line of the

* Trans. Inst. C. E., vol. iii. part 3, p. 219.

descending current, the platform would be acted upon with the greatest force from above. In the first case, therefore, the tendency would be to lift the platform, and in the second to depress it.

Again, supposing that there existed no natural impediments to the direct course of a current of wind, it would probably move forward nearly parallel with the surface of the water. If a suspension bridge obstructed this current, the wind so impeded would escape, partly over and partly under the bridge, but would possess no greater tendency to lift the platform than would result from the excess of the force arising from the compression of the air between the platform and the water's surface over that of the air above the platform; and if the areas of the impeding fronts of two bridges were similar, the difference in the amount of pressure would be greatest where the space between the platform and the surface of the water was least; or, in other words, the lifting tendency would be greatest when the platform was nearest to the water.

Conway Bridge not affected by Storms.

Experience has shown that suspension bridges which hang low are not acted upon by winds so violently as those of greater altitude, even where the bridges are constructed upon precisely similar principles. Conway Bridge is 327 feet between the centres of the supporting towers, and 15 feet above the level of high water: its chains, side railing, joists, floors, &c. were all constructed either from the same patterns or on the same principles as those of the Menai Bridge. Now Conway Bridge is not perceptibly moved by the wind, whilst that over the Menai is violently agitated, and it may therefore be inferred that if the disturbing force were uniformly most powerful from below, the former bridge would, in proportion to its size, be subjected to greater undulation than the latter: the reverse, however, is the fact.

Conway and Hammersmith Bridges compared.

Conway and Hammersmith Bridges are of nearly equal spans and heights above the water; but the construction of their platforms and the connections with the main chains are different. Conway Bridge is without trussed longitudinal frames, and the means of resisting undulation are the same as those of the Menai Bridge. The main chains of Hammersmith Bridge are in contact with the platform, and the latter has four lines of trussed longitudinal frames fixed vertically on its surface. Now though so differently constructed, neither Conway nor Hammersmith Bridges are affected by winds, and therefore, instead of attributing the stability of Hammersmith Bridge (as is done by Colonel Pasley) entirely to its trussed framings, the

steadiness of this, as well as of Conway Bridge, must be considered to be consequent upon their low and sheltered situations as well as to the diminished scale of these structures compared with that of the bridge over the Menai Strait.

Roof at Chatham destroyed.

The case which Colonel Pasley has alluded to of the roof being blown off one of the sheds in Chatham dockyard may very probably have been owing to the rebound of the wind from the surface of the ground, or it may have been caused by the wind having freer access on one side of the shed than it had means of escape on the other; but it by no means establishes as a fact that suspension bridges are always acted upon most violently from below.

Oscillation of the Main-chains, &c.

It has been previously observed that, in addition to the vertical undulation, both the platform and chains of the Menai Bridge were subject at times to considerable oscillation. In the hurricane of 1839 the main chains, which are suspended 12 feet apart, were, after the destruction of the transverse tubes and ties, thrown violently against each other. In 1836, the Anglesea end of the platform was blown 15 inches out of its proper position; and in 1839 the same end of the platform oscillated at least 3 feet from the vertical plane of suspension. All these facts lead to the conclusion that winds act strongly and prejudicially upon the fronts of suspension bridges as well as upon the horizontal surfaces of their platforms; and further, that the effect of winds is modified and varied by the shape of the country and the local circumstances connected with each individual bridge.

Colonel Pasley's proposed alterations to the Menai Bridge.

The remedies which Colonel Pasley proposed to apply at the Menai Bridge were:—

First.—"That a course of planking should be laid transversely over the existing roadway, which consisted of two courses of planking, both laid longitudinally."

Secondly.—"That two longitudinal lines of strong trussed railing, on the same principle as those of the Hammersmith Bridge, should be substituted in lieu of the former light network."

And thirdly.—"That two more lines of trusses, exactly similar to the former, and parallel to them, should be introduced in the intervals between the footpath in the centre and the carriage-ways on the sides of the bridge."

Though dissenting from Colonel Pasley's opinion as to the universal cause of injury to suspension bridges, I agree with him as regards the propriety of

giving increased longitudinal rigidity to their platforms when they undulate to any considerable extent. It has been already stated that I advised this to be done at the Menai Bridge in the spring of 1836, and again after the hurricane of 1839, before I had an opportunity of knowing Colonel Pasley's sentiments on the subject. There were these differences, however, in the details of our respective propositions:—The Colonel recommended the addition of a third course of planks laid transversely *upon* the two longitudinal courses, and also of four lines of trussed longitudinal framings;—my plan was to place the additional course of planks *between* the two longitudinal courses, and to give the requisite rigidity by two lines of longitudinal beams bolted together, instead of by trussed framings. My reasons for preferring beams to frames were, the facility with which the former could be increased in number if the degree of stiffness first obtained was not sufficient, and the difficulty which I believed would be experienced in keeping trussed frames at all times firm and in their true vertical positions.

W. A. PROVIS.

24, Abingdon Street, June, 1840.

XV.—*Account of the Alterations made in the Structure of the Menai Bridge, during the Repairs in consequence of the Damage it received from the Gale of January* 7, 1839.

By THOMAS JAMES MAUDE, Grad. Inst. C.E.

Read January 12, 1841.

The roadway of the Menai Bridge having sustained considerable injury during the hurricane of the 7th of January, 1839, it was considered necessary, after a minute examination of the structure, to replace the damaged platform by a new one. Instructions were accordingly given to Mr. Provis, C.E., by the Commissioners of Her Majesty's Woods and Works, directing him to carry into effect the re-construction of the platform, in conformity with the plans and estimates which he had submitted to the Board, and the author was subsequently appointed to superintend the execution of the works.

Instructions for Repairing the Platform.

Whilst the restoration of the platform was in progress, some modifications in its construction were adopted, which were suggested by the observation of the defects of the original roadway, and which the effects of the recent storm had rendered still more obvious. The nature and extent of these alterations are shown by the sections of the platform in its original state and after the restoration (Plate XVII.), and may be thus briefly described:—

Alteration of the original system of Platform.

First.—The roadway bearers were considerably strengthened, and an additional joint was introduced at the points of their suspension from the two middle chains, thus enabling the motion of the carriage ways and of the footpath to be independent of each other.

The reasons for this alteration are shown by Mr. Provis in his account of the Menai Bridge. After noticing the effects of a heavy gale, which occurred in February, 1826, soon after the bridge was opened, and describing the details of the construction of the roadway bearing-bars, Mr. Provis proceeds thus:—"Each long bar was thereby fixed at three points to the vertical suspending rods, and it was considered that this mode of fastening would tend very considerably to prevent any twisting of the roadway. It had that effect, but as it has been before observed that

Cause of Fracture of Roadway Bearing-Bars.

motion was principally communicated to the roadway by the vibration of the windward chain, that end of the roadway bar which was suspended from it was lifted up, and the other two points of attachment remaining nearly stationary, the bar became a lever with its fulcrum at the middle point of attachment, and the resistance to be overcome being greater than the strength of the bar, it consequently broke at its weakest point or fulcrum. Nearly fifty of the long bars were thus broken principally about half way between the pyramids and the middle of the bridge, those parts having been subjected to the greatest motion."

Mode of Repairing the Roadway Bars.

"To secure the fractured bars an iron stirrup was attached to the lower end of each of the vertical suspending rods, in the same manner as the roadway bars had been fastened, and a hole being drilled through each end of the stirrup and the corresponding ends of the fractured bars, they were bolted together and made perfectly secure."

New Form of Roadway Bars.

In order to prevent the recurrence of such accidents it was considered advisable, in re-constructing the platform, to adopt the new form of roadway bars shown in Plate XVII., by which arrangement each bar, being suspended at two points only, has perfect liberty to play, should the bridge be subjected to violent motion in any future storm. As a further security the depth of the bars was augmented by half an inch; and the strength round the eyes by which they were attached to the suspension rods was considerably increased, it having been found that the old bars were invariably broken across the eye.

Additional Joints in the Suspension Rods.

The additional joint introduced into each of the suspension rods near the floor of the bridge is the next modification to be noticed. It was found that the violent motion of the bridge caused by the storm of January, 1839, fractured many of the suspension rods at the surface of the platform, the planking through which they passed, acting as a fulcrum, against which they were broken in such numbers that a great portion of the platform was entirely torn from its fastenings on one side, and hung down flapping in the gale, only supported by one line of suspension rods. The joint now introduced immediately above the surface of the platform, will in future allow free play to the suspension rods at this point, and thus prevent their fracture. The undue motion of the bridge, as well lateral as longitudinal, is now guarded against by the joints being placed so as to work in an inverse direction to the play of the roadway bar on the one hand,

and to that of the rod immediately above the roadway on the other. Additional strength is given to the short suspension rod thus introduced, it being made $1\frac{1}{4}$ inch square, whilst the other rods are only an inch.

Repairs to the Platform.

The increased thickness given to the platform itself by the addition of a course of three-inch planking laid in a transverse direction is next to be considered. The planks are laid close together across the entire breadth of the bridge, being placed between the two longitudinal courses, which are of the same thickness as before. The transverse course of planking serves to bind the platform together in the direction of its breadth in the most perfect manner, accomplishing this object far more effectually than was done by the insulated oak planks, which were introduced between the roadway bars beneath the platform after the fracture of the latter in 1826. It also assists materially in imparting longitudinal rigidity to the platform, the three courses being firmly bolted together throughout the whole length of the bridge, at intervals of every $2\frac{1}{2}$ feet in each breadth of plank in the upper longitudinal course. It may be observed that the two courses of planking in the original roadway were only spiked together, which mode of fastening afforded much less firmness and solidity than the present compact bolting. The lower tier of planking is bolted to the roadway bars in the same manner as was formerly done. The oak-planks underneath the platform are now removed.

Method of fastening the Planking and strengthening it.

As a means of giving additional stiffness to the platform, and of checking undulation in a longitudinal direction, beams are bolted to the under side of each carriage road, as is represented in the section of the new platform. Each beam consists of two pieces of Memel fir 13 inches in depth by $4\frac{1}{4}$ in thickness, one on each side of the king-posts of the trussed roadway bearers; between these pieces an inch plank is introduced, which fills the intervals between the king-posts; and the top of the beams being notched up 1 inch on to the roadway bars, the remaining space to the lower side of the platform is filled up by a 3-inch plank, indicated in the section by dotted lines. The two lowest pieces are bolted together by $\frac{5}{8}$ inch bolts at intervals of $2\frac{1}{2}$ feet, and the whole is then secured to the platform by $\frac{3}{4}$ inch bolts, also at intervals of $2\frac{1}{2}$ feet, passing through the three courses of planking and the beams, and screwed up on the lower side of the latter. The two pieces forming the lower portion of these beams are so arranged that the joints on one side nearly coincide with the centres of the pieces on the other. The pieces are about 40 feet in length;

and the joints are further strengthened by a 3-inch plank about 6 feet in length, bolted on the side of the beam, and equally overlapping the joints: these pieces, to avoid confusion, are omitted in the section.

Alteration of the Wheel-Guides.

In order to gain a still greater degree of stiffness, the wheel-guides are increased in size; their height is 16 inches, and their breadth at the base 14 inches. These wheel-guides are composed of three distinct portions; of the two lower parts the one next the carriage-track is of African oak, which on account of its superior hardness is well calculated to resist the wear and tear arising from carriages, &c.: the outer lower portion, as also the cap or upper piece, is of Memel fir. The two lower pieces are bolted together with $\frac{5}{8}$ inch bolts at intervals of $2\frac{1}{2}$ feet: the caps are fastened to these, and the whole are fixed to the roadway by $\frac{3}{4}$ inch bolts, passing through to the bottom of the platform at the same distance from each other.

The lengths of the various beams, planks, and other timbers introduced throughout the whole structure, are so disposed that they break joint as regularly as possible. By means of the stiffening beams beneath the platform and of the wheel-guides above it, a total depth of 3 feet 4 inches from the top of the wheel-guides to the under-side of the beams is gained as a resisting power to the undulatory motion of the platform. In the original construction the depth, from the under-side of the floor to the top of the wheel-guides, was only about 1 foot 3 inches; and the action of the latter in resisting motion was very imperfect, in consequence of the short lengths of the beams which merely abutted at the joints.

Having thus endeavoured to describe the alterations which have been made in the construction of the platform, it remains to be observed that the preservation of that simplicity of construction, which is so striking a feature in the original design of the bridge, was one of the principal objects always kept in

Original Plan of Bridge adhered to.

view in effecting the alterations. In none of the improvements now introduced has that simplicity been interfered with. So that now, as formerly, should any accident occur to any part of the bridge, that part can be repaired or replaced without disturbing the other parts of the structure, and at the least possible expense of time or labour.

Weight of Timber and Iron-work.

The weight of the additional timber and iron-work recently introduced into the bridge is about 130 tons, still leaving the weight of the suspended part of the bridge far below that which the chains are estimated to be capable of bearing.

The whole of the timber has been Kyanised, and each course of planking coated with Archangel tar. Felt, however, has not been used, as was originally done between the tiers of planks, that material not appearing to have answered the intended purpose.

There have been no gales of sufficient violence to test the stability of the bridge since its restoration. During the progress of the work, however, about the end of January (1840), some high winds occurred: by that time the two lower courses of planking were completed, and a part of the third was laid down, the stiffening beams underneath were also in their places, but not yet bolted up to the platform; the wheel-guides were not yet on the bridge; but even in this imperfect state of the work, the effects of the wind were so slight, that it was evident the most beneficial results would follow the completion of the improvements.

It is satisfactory to state that the repairs have been effected without any inconvenience to the public, one or other of the roadways having been kept open during the whole time; nor did any accidents occur during the progress of the work, which was completed within the estimate furnished by Mr. Provis previous to its commencement.

THOMAS JAMES MAUDE.

Abingdon Street, Westminster, June, 1840.

XVI.—*Description of a Cofferdam adapted to a hard Bottom, used in Excavating Rock from the Navigable Channel of the River Ribble; designed for the Ribble Navigation.*

By DAVID STEVENSON, C. E., EDINBURGH.

Read February 23, 1841.

Usual construction of Cofferdams.

COFFERDAMS, constructed of timber piles driven into the ground by the piling engine being extensively employed in the hydraulic works of all countries, are familiar to every one. It occasionally happens, however, that the construction of cofferdams by driving piles in the manner alluded to is found to be impracticable, owing to the hardness of the soil in the bottom of the sea, river, or lake, as the case may be, in which they are to be erected, and in such situations some modification of the ordinary apparatus is rendered necessary. The cofferdam described in the following pages was designed with the view of overcoming some formidable difficulties, which presented themselves in executing the works at present in progress for the improvement of the navigation of the Ribble, and was erected in a part of that river where the bottom consists of sandstone rock, and to which a cofferdam of the ordinary construction was consequently quite inapplicable.

Improvements of the Navigation of the river Ribble.

Before explaining the construction of this cofferdam, however, it may not be out of place to give a brief sketch of the projected improvements of the Ribble navigation, in effecting one department of which, the apparatus about to be described has been successfully employed.

Description of the Ribble.

The river Ribble, in the county of Lancashire, is the natural outlet for the trade of the large manufacturing town of Preston, which stands on its north bank, at the distance of about 15 miles from the Irish Sea. But the geological formation of the bed in which the river flows, consisting alternately of solid sandstone rock, compact gravel, and loose sand, renders the present state of its navigation very imperfect; *flats* or *keels* of small draft of water being the only craft which can safely and regularly approach the quays of Preston.

State of its Navigation.

Impressed with the great importance of improving the navigation of the Ribble, and facilitating the communication with the sea, the inhabitants of Preston resolved to take some vigorous measures in order to bring about

the attainment of this object. A Company had been formed so long ago as 1806, which, under the Act of Parliament then granted, had at various times made some improvement in the navigation; but the resources of this Company having fallen greatly short of the object which its shareholders had in view, the Corporation of Preston in 1835, consulted Messrs. Robert Stevenson and Sons of Edinburgh, as to the practicability of improving the navigation; and the opinion expressed, after completing the survey and examination of the river, being favourable, a subscription was immediately commenced for carrying the plans proposed by them into effect, and for obtaining an Act of Parliament for the formation of a Joint Stock Company, to be called the "Ribble Navigation Company." Satisfactory arrangements having been entered into with the shareholders of the former Company, the Bill as amended received the Royal Assent in May 1838; and preparations for effecting the improvements in the navigation having been commenced without loss of time, the operations forwarded by the zealous exertions of Mr. Haydock the chairman, and the directors of the Company, are now proceeding expeditiously and satisfactorily.

Ribble Navigation Company formed.

Description of the Works.

The works at present in progress of execution are the excavation of rock from the bed of the Ribble, the removal of gravel and sand by steam-dredging, and the formation of low rubble walls, for the purpose of directing the low water channel of the river at a part of its course where the configuration of the banks renders this operation necessary, in order to obtain a permanently straight navigable track for shipping. It is, however, only on one of these departments, namely the rock excavation, that I am induced to offer a few remarks; as I believe, from the extent of rock which has been removed and from the simple means by which the excavation has been effected, that this work may be considered somewhat novel in its nature, and that a brief account of it may prove interesting, and perhaps instructive, to those who may happen either now or hereafter to be engaged in similar undertakings.

Extent of Red Sandstone Rock.

About half a mile below the quays of Preston, a solid bed of red sandstone, upwards of 300 yards in length, stretches quite across the bed of the Ribble. The position of this band of rock, which extends inland under both banks beyond the limits of the river, is shown in the Plan and Section Plate XIX., Figs. 1 and 2. Its surface is on a high level compared with the general bed of the river both above and below it, and where it is washed by the stream it is quite bare, and free from any deposit of sand or mud, and the higher

parts are occasionally left dry during the long droughts of summer. The influence which this band of rock exerts in distorting the tides and currents of the river can be viewed in no other light than that which would be produced by a great artificial weir thrown across the stream. The free flow of the river and tides being checked on approaching it, the power of the tidal and fresh water scours is thereby neutralized, and rendered almost unavailable in keeping open the upper and lower stretches of the navigation. In proof of this it may be stated, that the tide at Lytham, which is situated near the mouth of the river, and is 12 miles distant from Preston, was ascertained by Captain Belcher, R.N., while engaged in making the Admiralty survey of the lower part of the Ribble, to have risen on one occasion, in July, 1836, no less than 25 feet 7½ inches. The ordinary rise of spring-tides, however, at that place is about 19 feet, and that of neap-tides 14 feet; whilst at the quays of Preston, the ordinary rise of spring-tides did not exceed 6 feet, and neap-tides of 14 feet rise by the Liverpool Tide Tables did not show at Preston at all previous to the commencement of the operations. Such was the scarcity of water and the imperfect state of the navigation in the upper part of the Ribble; and the removal of the rock which encumbered its bed was naturally viewed as the most urgent and important work for effecting an improvement.

Impediment to the Navigation.

Height of Tides.

The old Navigation Company already alluded to had, by the erection of low tide-dams, succeeded in excavating a channel about 70 feet in breadth and from 3 to 4 feet in depth in this band of rock. But that operation was too limited to meet the views of the new Company; the directors of which resolved to carry the works of excavation to a great extent, both as regards depth and breadth, and after due deliberation, determined to excavate a channel 100 feet in breadth, as shown by the cross section, Fig. 3, Plate XVIII., and affording a navigable depth of 20 feet at high water of spring-tides through the whole extent of the rock, which, as formerly mentioned, is upwards of 300 yards in length.

Improvements previously effected.

It had been suggested that a new cut might be formed and the course of the river *permanently* diverted so as to avoid the rock entirely; but from the formation of the banks, as ascertained by minute examination, as well as from the nature of the adjoining property, it was concluded that under all circumstances no advantage could be obtained by the proposed diversion, and it was accordingly resolved, in commencing the operations under the

Diversion of the Channel proposed.

new Act, to keep to the old course of the river as the track for the improved channel, and to limit the breadth of the excavation in the first instance to 100 feet, so as to leave ample space for the free flow of the river and an uninterrupted navigation during the progress of the work; the excavation of the remaining part of the rock being a work which may be undertaken at any future period, should the trade of the port render it necessary to provide a broader channel.

The best means of executing this work, involving an excavation in solid rock at some places not less than 13 feet 6 inches in depth, in the bed of a rapid river liable to sudden and extensive land floods, engaged a good deal of attentive consideration, and after weighing the merits of various expedients, such as the use of the diving bell and the formation of a new cut so as to obtain a *temporary* diversion of the stream, whilst the work was going on, it was at length resolved to make use of a series of cofferdams as the most efficient and economical method of effecting the excavation.

Difficulty of fixing Cofferdams.

The chief obstacle which presented itself in designing a cofferdam for this situation, was the difficulty of obtaining on a rocky bottom (covered with water varying from 2 feet to 10 feet in depth at low water and from 8 feet to 16 feet at high water) fixtures of a nature sufficiently secure, to enable the cofferdam to resist the rapid currents which occur during floods and high tides; whilst the inequalities of the bed of the river occasioned another difficulty in attempting to form a water tight joint between the bottom of the cofferdam and the irregular surface of the rock. As the water way of the river also, so long as the dam remained in its bed, would necessarily suffer a very considerable reduction in its breadth, it was considered highly important, as well for the trade of Preston as for the safety of the work, to construct a cofferdam which would occupy as little space, and present as little interruption as possible to the free flow of the currents and the passage of vessels. The construction which I designed for executing this work has now been in use for upwards of twelve months, and has been found fully to answer all of these conditions, the fixtures in the rock having proved quite sufficient, and the dams being exceedingly tight, while the space occupied by the walls or sides is very small considering the water-pressure to which they are occasionally exposed.

Description of Cofferdam.

The Elevation, Plan, and Section, Figures 4, 5, and 6, represent the cofferdam exactly as it was executed, a few improvements in the details of its construction having been from time to time introduced by Mr. Bond, the contractor for the work. It will be seen that the cofferdam

consists of a double row of iron rods $2\frac{1}{2}$ inches in diameter, placed 3 feet apart, the spaces between the rods which form each row being 3 feet also. On the inner side of each row of rods, linings of three inch Memel planking are placed; and the space between these linings of planking which form the two sides of the cofferdam is carefully filled with well wrought clay puddle. The sides of the dam are kept together by bars of iron connected to two horizontal wale pieces of Memel timber, measuring 10 inches by 6 inches, placed on the outside of the iron rods. These iron bars pass horizontally through the heart of the puddle at proper intervals, and serve to counteract the tendency which the puddle exerts to force the iron rods and planking outwards, and thus to derange the whole structure. A row of strong stays, placed 18 feet apart from centre to centre, as shown in the Plate, is also applied in the inside of the dam. To avoid interrupting the navigation, as well as for greater safety, the dams were stayed entirely from the inside. These stays, as shown in the drawing, have joints at the upper extremities, and being simply slipped over the tops of the iron rods, and kept in their places by *cotters*, their lower ends, which rest on the bottom, can be moved either horizontally or vertically, and thus be easily adapted to the level of the rock. The shorter stays applied in the first instance can be removed as the work proceeds by simply driving out the *cotters* at the tops of the iron rods, and their places supplied by longer stays resting on the bottom of the excavation, as shown in dotted lines in Figure 6. A sluice, at the level of low water, which can be opened so as to admit the water and prevent the dangerous consequences of a sudden rising of the river while the interior of the dam is empty, two cast iron pumps of 12 inches bore, with their gearing, and a steam engine of 10 horses-power for pumping the dams dry, complete the whole apparatus.

Means of strengthening the Dams.

Mode of fixing the Iron rods.

In constructing the dams according to this design, the most tedious parts of the operation were those of fixing the iron rods into the bed of the river, and securing the lower tier of planking which rested on the irregular surface of the rock. The manner in which these operations were effected I shall endeavour briefly to explain.

In order to fix the iron rods, a jumper point was first worked on the end of each of them. They were then successively *jumped* into the bed of the river to depths varying from 12 inches to 18 inches, according to the soundness or hardness of the rock, by labourers who worked from punts moored in the line of the dam, three or four men being employed at each rod. Gauges were

used for enabling the workmen to *enter* the rods properly, so that they might retain a nearly perpendicular position when fixed, and also for the purpose of preserving the proper line of the dam and placing the rods at equal distances apart. No other fixture than that produced by simply jumping the rods into the rock was applied, but this was necessarily a tedious process, from the difficulty of working in a rapid run of water, and from the repeated interruptions which occurred, occasioned by the rise of the tides and by land floods. When a sufficient length of rods had been fixed in the manner described, the lower tiers of planking, which were to be placed below the level of the water, were secured to the iron rods by clasps of iron, as shown in the drawings, and slipped down into their places one above another. The under edge of the lowest tier of planking, the fitting of which often occasioned much trouble, was cut, previously to being put down, as nearly as possible to suit the inequalities of the rock, which were ascertained approximately by measuring from the surface of the water down the iron rods to the bed of the river. The plank being then lowered into its place, a small iron rod, with a hooked end which could go under the plank, was used for finding what parts of it did not touch the rock, and this having been ascertained, the plank was raised and again cut. This operation was repeated two or three times until a near approach to the contour of the rock was obtained. The lower edge of the plank was then cut with the adze in a bevelled or wedge-shaped form, and the plank, being finally lowered into its place in the bottom, was beaten down by blows from a heavy mallet upon an upright piece of wood resting on the upper edge of the plank, and extending above the surface of the water, and the sharp bevelled edge yielding to the blows, sank into the smaller irregularities of the rock, and thus ultimately, as experience proved, formed, in connexion with the puddle behind it, a perfectly water-tight joint. The planks above low water had no fixture to the iron rods, and were kept in their places simply by the pressure of the puddle in the inside of the dam.

Mode of fitting the Planking immediately in contact with Rock.

Method of removing Cofferdams.

In removing the dams from the bed of the river at the completion of the excavation, the clay and planking were first taken up, and the rods being then moved to and fro could generally be raised after a little trouble, by men who worked from punts, as in putting them down. When the rods resisted this force, however, which not unfrequently happened, they were made fast at low water to cross bars resting on two punts, which, being

gradually raised by the flowing tide, threw a great strain on the rods, and they were then easily sprung from their fixtures in the rock by a few blows from a heavy hammer. Some of the iron rods were unavoidably a good deal bent in being raised; but most of them, when taken up, were found to be ready, without repair, for being used in forming the next stretch of dam.

Cases in which this kind of Dam was used.

It may be proper to remark that this dam was used only in those parts of the river where the rock was kept bare by the scour of the river and tide, or was covered to the depth of 8 or 9 feet with gravel. In the latter case the gravel was dredged out and the rods fixed in the bottom as already described. But wherever the deposit on the rock exceeded the depth of 8 or 9 feet, as at the upper and lower extremities of the excavation, recourse was had to a timber dam, constructed in the ordinary manner with gauge and sheeting piles of timber driven into the bottom by the piling-engine. The execution of these parts of the cofferdam, however, being in no way remarkable, I shall only in conclusion state briefly the extent of work executed by the means which I have described, my information on this point being extracted chiefly from the monthly reports and measurements of Mr. Brebner, the former, and Mr. Park, the present resident engineer for the navigation works.

Extent of Excavation performed.

The whole extent of the excavation, as already noticed, is upwards of 300 lineal yards; and it was determined to execute it with three lengths of cofferdam, as shown in Figure 1, (Plate XVIII.,) which is a plan of part of the river. The first stretch of dam, which was 350 feet in length and 120 feet in breadth, and enclosed an area of 4,666 square yards, was completed and pumped dry in October, 1839. A large force of men was employed in the excavation, and by working with a night and a day gang, the Contractor

Quantity of Rock raised in 24 hours.

succeeded, when the weather was favourable, in raising no less than 185 cubic yards of rock during 24 hours. The whole quantity excavated from this stretch was 10,714 cubic yards; but, from the unfortunate state of the weather during the winter, the excavation was not completed till February, 1840. The second dam, which was constructed of the same materials that had been used in the first, was placed so as to enclose the lower end of the first stretch within its limits, in order that the rock on which the first dam stood might be removed. This stretch, which was the largest, measured 384 feet in length and 130 feet in breadth, and enclosed 5,546 square yards, being an area of one acre and twenty-three poles. It was pumped dry in

April, 1840, and the excavation, which amounted to 12,449 cubic yards, was completed in the month of June of the same year. The third stretch of dam, which is about 300 feet in length, 130 feet in breadth, and encloses an area of 4,333 square yards, was pumped dry on the 1st of November, 1840, and the excavation is rapidly proceeding. The whole quantity of rock removed when the work is completed will amount, it is expected, to about 31,000 cubic yards.

The soft nature of the rock rendered it unnecessary to have recourse to blasting in forming the excavation, as it was found to yield easily to the quarriers' tools. The greater part of the stone excavated being loaded on punts was floated down the river, and used in forming the walls for the direction of the lower channel, already alluded to in speaking of the navigation works generally.

It was hardly to be expected that a series of cofferdams, exposed for so great a length of time in the bed of a rapid river, would escape the casualties attending such works, and some accidents accordingly occurred on the Ribble, which, however, were happily attended with no more serious consequences than the delays which they occasioned. It was satisfactory to find that the fixtures effected by merely jumping the iron rods into the rock in the manner described, the sufficiency of which I considered as somewhat problematical, proved, without a single exception, to be perfectly secure. The best proof indeed which can be adduced of the strength and efficiency of the whole structure is the fact of the river's having risen, on one or two occasions, some feet above the top of the dams without injuring them in the smallest degree. I had an opportunity of seeing the river in this state in the month of March, 1840, when on a visit to the works. At that time no part of the dams was visible, the tops of the iron rods being quite submerged, while the velocity of the current which was rushing over them could not be less than five miles per hour. Notwithstanding this great trial, I had the gratification of hearing from Mr. Park a few days afterwards, that the water having subsided, and the pumps having been set on, the dam was again perfectly dry and the men busily at work.

DAVID STEVENSON.*

Edinburgh, 1st December, 1840.

* Since the preceding paper was written, the last stretch of excavation has been completed, and the whole of the cofferdam has been removed from the river. The total quantity of rock excavated was 30,793 cubic yards, and the time occupied in the execution of the work, including all interruptions occasioned by floods and other causes, was about 18 months.—D. S.

Edinburgh, 3rd June, 1841.

XVII.—"*Memoir on the Practicability of Shortening the Duration of Voyages, by the Adaptation of Auxiliary Steam Power to Sailing Vessels.*"

By SAMUEL SEAWARD, F.R.S., M. Inst. C.E.

Read February 9, 1841.

Application of Steam Power to Navigation.

A QUARTER of a century has barely elapsed since the application of steam power to the purposes of maritime and river navigation was regarded more in the light of an ingenious problem than as a practicable undertaking; the perseverance and skill of scientific and enterprising men have, however, triumphed over the difficulties which beset them, and the result is that steam power has not only been rendered subservient to coast and river navigation, but by its means the shores of the Mediterranean and of the Atlantic are approached and those seas traversed with celerity and certainty, in defiance of winds and tides: it may now, therefore, be presumed that steam navigation is permanently established, though at present not so extensively as the exigencies of maritime transport require.

Limit of duration of Steam Voyages.

Notwithstanding the numerous improvements which have been made in the form and dimensions of the hulls of steam-vessels, and the perfection to which the machinery has been brought, still the weight of the latter, together with the space required for the fuel, has rendered it hitherto impracticable to extend the duration of a steam voyage beyond the period of twenty days, without the necessity of taking in a fresh supply of coals. It must therefore be concluded that until some great reduction can be made in the weight of the engines and in the amount of fuel required to keep them in motion, the use of steam as the sole moving power must be limited to voyages of three weeks' duration, a period of time wholly inadequate for the performance of an Indian or South American voyage.

Cause of limited application of Steam in Sea Voyages.

A practical examination of the cause of the limited application of this noble invention shows that every attempt to adapt engines to vessels of a large size, so as to obtain increased space with

a degree of speed proportionate to that of smaller boats, is frustrated by the invariable result, that the weight of the enlarged engines is greater in the proportion of about 25 per cent. than that of the smaller engines; and that whilst the machinery of a boat which has an engine of only 30 horses-power, weighs, including the boiler, water, &c., only 30 tons, or 1 ton per horse-power, the engines of a ship having 300 horses-power will weigh 375 tons or $1\frac{1}{4}$ ton per horse-power. The immediate consequence of this disproportionate increase of weight is a greater immersed section of the vessel, and a necessary increase of the resistance offered to her motion, so that the force required to propel her through the water must always increase with the power employed, and not even cubing the steam power would produce double the velocity. The experience of persons acquainted with steam navigation has determined, that if a vessel be propelled by the application of steam power at the rate of 8 miles an hour through the water, no multiplication of that power could double her speed: it is further a matter of great doubt whether, supposing a vessel be propelled at the rate of 8 miles an hour by an engine of 200 horses-power, an additional speed of $1\frac{1}{2}$ mile per hour could be given to her by the application of 400 horses-power.

Weight and bulk of fuel a further cause of limit.

Independently also of this question, though intimately connected with it, is the consideration of the additional weight and bulk of the fuel required for the increased consumption of the enlarged engines, and which is found to be in the same ratio with the increased power; so that if 150 tons would suffice for 10 days' consumption of a boat with engines of 200 horses-power, 300 tons would be required for the same length of time in a boat with 400 horses-power; and the conclusion to which these statements inevitably lead is, that if a vessel of 200 horses-power can be propelled at the rate of 8 miles an hour by an expenditure of 15 tons of coals per diem, the same vessel with 400 horses-power, and a consumption of 30 tons per diem, would only obtain a speed of $9\frac{1}{2}$ miles per hour, an increase in velocity quite disproportioned to the means by which it is effected.

A third, and, in a commercial point of view, a most material circumstance affecting the utility and economy of steam-vessels of great power, is the space necessarily occupied by the engines and the supply of coals, which are found to require three-fourths of the whole area below deck, leaving only one quarter for the stowage of cargo; and that, owing to the great weight of the former, must principally consist of measurement goods. Thus the "Presi-

dent," and the "British Queen," although of 2000 tons register, have never been able to carry more than 500 tons of measurement goods as freight.

From a due consideration of these three features of the question, with respect to the expediency of employing large steam-vessels, (and it is believed that few practical men will dispute the facts by which they are developed,) it is evident that the attempt to perform voyages of lengthened duration by the power of steam alone must, in the present state of engineering science, be attended with an expense wholly disproportioned to the profits, when the cost of the hull and engines, the consumption of fuel, the wages of the engineers, stokers, &c., are compared with the amount of freight.

The question next to be examined, therefore, is the extent to which steam power may be profitably employed in shortening the duration of commercial voyages; for, with respect to passengers and mails, it is conceived they do not enter into the category of considerations proposed by the author of this essay.

Diminution of Power of Engines recommended in some cases.

A strong and at the same time an apparently well-grounded opinion is entertained by the author, that in many cases steam-vessels are actually overburthened by the size and weight of their machinery, and that the speed of several steamers now afloat might be materially improved, as well as a considerable saving effected in their wear and tear and the cost attendant on them, by reducing the power and weight of their engines. A recent case, which is familiar to the profession, affords an apt and strong proof of the truth of this opinion; and it may be cited with advantage as illustrative of a point in steam navigation, which has hitherto not received quite so much attention as it merits.

Advantages of diminished Power, illustrated by the "Liverpool."

The "Liverpool," constructed at the port after which she was named, in the year 1837, for Sir John Tobin, was considered to be a perfect model of an Atlantic steamer, her beam being only 29 feet to a length of 220 feet, whilst her sharp bows and clean run aft were calculated, it was anticipated, greatly to increase her speed. The measurement of the "Liverpool," according to the old rule, would have given her a register of 1000 tons, but this burthen was virtually diminished to 900 tons by the sharpness of her bows and stern, which lessened her stowage to the extent of 100 tons. Into this vessel, so constructed, engines of 450 horses-power were placed, thus giving her exactly one horse-power to each two tons burthen, which was the greatest proportion of power to tonnage that had up to that period been adapted to a steam-ship. The weight of the engines, together with that of the

boilers when filled with water, brought the hull of the vessel down to within two feet of the estimated water-line with her coals and cargo on board; but when 500 tons of fuel (a short supply for 18 days' consumption) were added to the weight already on board, she sank so deep that her paddle-wheels became immersed 4 feet below their proper dip, and the action of the engines was very much impeded, occasioning great waste of power.

The "Liverpool" subsequently made two or three voyages across the Atlantic in a very indifferent style; she was then taken off the line, having proved altogether unfit for the purpose for which she was built.

It may fairly be estimated, that had engines of 300 horses-power been placed in this vessel, instead of those of 450 horses-power, her speed would have been materially increased, as the full force of the engines could have been exerted, and the diminution of one-third in the quantity of fuel required, as also one-third of the weight of the engines, would have eased her of a burthen of upwards of 300 tons, and have kept the vessel in fair trim for sea-going purposes.

The correctness of this supposition may be inferred from the result of the alterations which were made subsequently at a great cost, in the form of the hull of the "Liverpool." Upon being taken off the line of Atlantic steamers, she was docked at Liverpool, the whole of her sides were removed down to the bilges; futtocks and new sides were worked up to the same height as her former bulwarks, with an additional width on each side of 3 feet, thus changing a vessel of 29 feet beam into one of 35 feet, and giving her thereby an additional burthen of 400 tons, increasing the register from 900 tons up to 1300 tons.

The improvement which resulted from these judicious alterations, not only in her speed, but also in her sea-going qualities, was very obvious during her passage to Alexandria, and her performances have since been satisfactory and regular, the proportion between the tonnage of the ship and her steam power being now as one to three and a quarter, instead of, as before, one to two.

Ratio of Steam Power to Dimensions of Vessel.

The author is enabled, from his own observation, to mention another interesting case illustrative of the opinion which he entertains with respect to the ratio which steam power and the dimensions of vessels ought to bear, one to the other.

The "Gem," Gravesend steamer, was fitted in 1836, by Messrs. Seaward and Co., with a pair of engines of 50 horses-power; the vessel was 145 feet long with 19 feet beam: when she was tried, her speed fell short of the

expectations that had been previously entertained, and it was seen that the cause arose from the weight of the engines, which overloaded her and brought her too low down in the water: the utmost speed ever attained by the "Gem" with these engines was 12½ statute miles per hour. After the season was over these engines were taken out of the "Gem" and placed in the "Ruby:" the latter vessel was 5 feet longer and 9 inches broader than the former, and consequently possessed a greater degree of buoyancy. Upon the first trial the speed of the "Ruby" was found to be 1 mile an hour greater than that of the "Gem," her velocity being 13½ miles an hour. A pair of engines of 45 horses-power was then placed in the "Gem:" the immediate result of this was, to lessen her draught 5 inches, and to increase her speed from 12½ to 13 miles an hour.*

It is conceived that these two cases exemplify, in a striking manner, the opinion before stated; but at the same time it must not be assumed that, in quoting them, the author condemns the employment of a high degree of power on board steam-vessels. The object which he proposes to effect in making these observations, is to demonstrate what he conceives to be an undeniable fact, namely, that, under the present circumstances of steam power, vessels are frequently overburthened by their engines and fuel; but at the same time, if increased weight and space were not the inseparable concomitants of increased power, it is admitted that there would be no possibility of assigning any real limits to the employment of this means of propelling ships. It may also be observed that those engineers have succeeded best who have studied how to combine diminished weight with equal strength in the construction of their machinery, by the judicious choice and application of materials.

Classes of vessels in which Steam is used as the sole moving power.

Taking therefore a general view of these facts, it may be stated that the vessels in which steam is employed as the sole moving power must be ranged under three distinct classes:—

First: Those of the largest class, such as the "President," the "British Queen," &c., which from their size and capacity can carry a supply of fuel enabling them to undertake a voyage of the duration of twenty days, which forms at present the boundary of steam voyages.

* Since the above was written, the Ruby has had her bow lengthened, and a boiler placed in her weighing, with the water, 22 tons, instead of the former one, which weighed 44 tons: this has diminished the draught of water 4 inches, and has increased her speed ¾ of a mile per hour; her ordinary velocity through still water being now 14¼ miles per hour.

Secondly: Those which are able to carry a supply of fuel sufficient for twelve days' consumption; and lastly, those which cannot carry more than eight days' fuel. Of the first class there are only five: of the second class there are about thirty: of the number of the third class no exact estimate can be formed, but it must be observed that the majority of steam-boats fall into a rank much below this mark, not being able, generally speaking, to carry more fuel than is sufficient for three or four days' consumption.

Present state of Steam Navigation inadequate to the purposes of Commerce.

The present state, therefore, of steam navigation is evidently altogether inadequate to meet the large and daily increasing demands of commerce, and in this respect as far as maritime navigation (as contradistinguished from river and coast navigation) is concerned, steam must be considered to be in its infancy; for the communication by this means with our own, or with foreign distant colonies,—India *viâ* the Cape of Good Hope, the West Indies, the Pacific Ocean, Australia, the Brazils, and other highly important productive commercial countries,—is at present impracticable to any extent, as the facts hereafter stated will more distinctly show.

Steam as a sole moving power unfit for long Sea Voyages.

It is moreover to be apprehended that the difficulties which have hitherto been found to impede the direct communication, by means of steam power only, with these distant parts of the globe will be found to be insurmountable, and that therefore with respect to economy of time as well as of space, fuel, outlay, &c., some intermediate plan must be resorted to, before any profitable result can be looked for from the employment of this power.

Junction of Steam Power with Sailing recommended.

This intermediate plan is to employ the wind, with the common agency of sails, as the principal moving force, and to use steam power only in those cases, where the failure of the wind or its being contrary, would cause delay on the voyage; and thus by the combination of these two moving forces, to ensure dispatch, safety, and economy, and open a wide field for British skill and enterprize.

This plan was first suggested by the author in a pamphlet published in the year 1829.*

Although, during the interval which has since elapsed, the views therein exposed have been fully established, little has hitherto been done towards carrying them into that full and complete operation which the success attendant

* "Observations on the Advantages and Possibility of successfully employing Steam Power in navigating Ships between this Country and the East Indies."—London, 1829.

upon their partial application has, it is conceived, entitled the author to expect.

The "Maria," first example of combining Steam power with Sailing.

The earliest attempt to adapt steam power to a sailing-vessel was made in the year 1839, when the ship "Maria," of 460 tons burthen, was fitted with engines of 20 horses-power as auxiliary steam machinery, by Mr. John Melville; and in the years 1839 and 1840 two other vessels have had auxiliary steam-engines adapted to them, so that the experiment may now be said to be undergoing a fair course of trials.

The suggestions contained in the pamphlet above referred to (a copy of which, accompanied by a chart of the route to India and China, has been presented by the author to the Institution) were, that vessels of from 1500 to 1800 tons burthen should be constructed to sail 10 or 11 knots an hour before the wind, to be rigged and equipped in the same manner as ordinary Indiamen; engines of small power were to be placed in these vessels, to be brought into action only during calms, or to assist the sails under light airs; by this means it was calculated that the duration of the voyage to India would be decreased to seventy-five days, allowing seven days for stoppage at the Cape of Good Hope to take in water, coals, and provisions; whereby a saving of thirty-five days in the passage to India would be effected, one hundred and ten days being the average duration of the voyage.

Class of vessels best suited for combining Steam power with Sailing.

Before entering upon the details of this scheme, either as regards its past success in the partial and limited application which it has hitherto received, or as to its future adaptation, it may be desirable to describe the class of sailing-vessels, to which auxiliary steam may be regarded as an applicable power, and where its employment may be expected to be attended with success; and here it must be observed that commercial, and not scientific success, is the only result looked to: for nothing can be considered successful, in a commercial point of view, to which a fair profit upon the capital employed does not attach.

Auxiliary steam, then, may be considered to be applicable to all well-built vessels ranging from 400 tons upwards, wherein the engines can be placed so as not to encroach upon the stowage. On or between the decks of vessels of this tonnage sufficient space exists to contain power equal to the effort of propelling a ship at the rate of 5 knots an hour in a calm, the power necessary to attain this degree of speed being in the proportion of about one horse to twenty-five tons; the only stowage-room required would be for the coals neces-

sary for the consumption of the engines, the quantity of course depending on the nature and duration of the voyage.

Number and class of Vessels to which the Author's plan is peculiarly suited.

The number and classes of sailing-vessels in our commercial navy to which auxiliary steam might be at once advantageously applied, may be thus roughly estimated:—

East India Traders about	250	sail	averaging 800	tons each
Australian and New Zealand Packets	60	,,	600	,,
West India Traders	300	,,	500	,,
Other Merchant Vessels	1000	,,	400	,,

To all these it is conceived that auxiliary steam power may be applied with a certainty of its success in shortening the duration of voyages, and consequently of proportionately reducing the expense.

Usual speed of Sailing-vessels.

The generality of merchant vessels, particularly those which trade round the Cape of Good Hope and Cape Horn, sail before the wind, upon an average, at the rate of from 11 to 12 nautical miles an hour; in a gale this speed is increased to 13 or 14 miles, being from 2 to 3 miles an hour faster than any ordinary steamer could be propelled under similar circumstances. This result is occasioned by two causes, first by the impediment to the steam-vessel's progress through the water arising from the trailing of large wheels; and secondly because the masts and spars of such ships are so light, that the same spread of canvass and the same wind which would propel an ordinary sailing-vessel at the rate of 13 or 14 miles an hour would destroy the masts and sails of a steamer. It has, moreover, been found, by experience, to be extremely hazardous to use the full power of steam, when the speed of the vessel under canvass amounts to 9 miles an hour; so that under the most favourable combination of circumstances the utmost average speed that can be obtained by a steamer is 12 miles an hour, whilst with a wind on her quarter or abeam, her crank build reduces her way to 6 or 7 miles an hour; a sailing-vessel with the same wind, and holding on the same course, making 11 or 12 miles: even when a sailing-vessel is close-hauled or steering within five points of the wind's eye, she will make from 6 to 7 knots, if there be any wind amounting to a breeze, so that a sailing-vessel can always steer twenty-two out of the thirty-two points of the compass, with a way varying (according to the degree to which she is close-hauled, and to the amount of wind) from 5 to 12 or 13 knots an hour.

Average speed of Steam-vessels.

Advantages of the Author's plan in the Trade Winds.

It should also be borne in mind that in the Atlantic, Pacific, and Indian Oceans, there always prevail what are termed trade winds, which blow with varying force, in one constant direction within certain parallels of latitude, all the year round, and it is by the knowledge and skilful use of these winds that our Indiamen are enabled to perform their voyages so quickly as they do. The detention experienced on an average by these vessels in consequence of calms and contrary winds, amounts to about twenty-five days, which deducted from one hundred and ten days, the average duration of the passage, leaves the period of eighty-five days for the performance of the distance which the vessel has to traverse before she reaches her destination, giving an average speed of from 7 to 8 knots an hour, the actual speed, however, varying from 3 to 12 or 13 knots, according to the force and direction of the wind.

Causes of the lengthened Voyages of Vessels using Steam only.

Hence it is that the duration of the voyages of steam-vessels of great power which have been sent out to India has in every instance been lengthened to the extent of one-fifth beyond the average period occupied by sailing-vessels in making the same passage. The reasons for this are obvious, for, in the first place, the full power steamers, as now built, are bad sailers, their spars and rigging being very inefficient; consequently they rely almost wholly for their progress upon the power of steam, which, as has been shown, is not equal under certain, and not uncommon circumstances, to the power of the wind. Steam-vessels constructed like those which have hitherto gone round the Cape, can only run fourteen or fifteen days before they are compelled to put into port for fuel, a necessity which the length of the voyage to India obliges them to submit to four or five times, whereby many days are lost, during which the sailing-vessel is steadily pursuing her course: the time thus expended may be fairly balanced against that during which the sailing-vessel is becalmed or retarded by adverse winds, and the duration of the voyages to India (viz. from 130 to 150 days) of the steamers "Enterprise," "Atalanta," "Berenice," and others can therefore be amply accounted for.

Results of the employment of Steam as an Auxiliary Power, exemplified by the "Vernon."

Having thus weighed the advantages and drawbacks attendant upon both systems of navigation as at present practised, and having compared the powers of steam with those of sailing-vessels, it may be considered expedient to state the results that have been obtained by the employment of auxiliary power which has been adapted to the "Vernon" East Indiaman.

This vessel, measuring 1000 tons, was built by Mr. Green, of Blackwall, in the year 1837, and during one or two passages to India, proved herself to possess first-rate sailing qualities. Her hull is constructed much upon the model of a frigate; her masts are large and taunt, her yards square, with capacity for spreading a great breadth of canvass, and she sails with ease twelve to thirteen knots in a fresh gale. The "Vernon" therefore presented peculiar qualifications for the application of the auxiliary steam power, and as Mr. Green was fully impressed with the necessity of endeavouring to shorten the duration of sailing voyages round the Cape to India, in order to enable his vessels to compete with the Red Sea steamers, he readily adopted the suggestion which was made to him for applying auxiliary steam to this ship, and gave an unlimited authority to Messrs. Seaward, to equip the "Vernon" with the engines and machinery which they might deem necessary to effect the purpose required. Considering it to be a matter of great importance that the smallest possible steam power should be employed, Messrs. Seaward constructed a condensing engine of 30 horses-power, which with its boiler, &c., was placed midship between decks, occupying the space between the main and fore hatchways, the whole apparatus being comprised within the dimensions of 24 feet in length and 10 feet in breadth; no part of the engine going into the hold, which was filled with cargo in the ordinary manner. The total weight of the machinery was 25 tons; the cylinder was placed horizontally, and in order to economise space the shaft was fixed behind the cylinder bottom, and the two side rods from the piston cross head became the connecting rods to the shafts. The paddle wheels and shafts rested upon bearings placed in the bulwarks, from which the paddle wheels projected 5 feet on either side: there were no paddle boxes or beams, the whole apparatus being left open, and the wheels were so constructed that they could each be separated into two pieces and taken on board, leaving the ends of the shafts projecting about 18 inches from the ship's sides. The diameter of the wheels was 14 feet, the floats being constructed to reef to the extent of 18 inches, according as the dip of the wheel was altered by the varying draught of the vessel whether fully or partially laden. An apparatus was also attached to the engine by which the paddle wheels could be disengaged from the main shaft in the space of one minute, whenever that operation was necessary; a very important feature it must be observed in the adaptation of auxiliary steam to sailing-vessels, as it is essential that the engine should be stopped whenever the breeze which it is intended to assist freshens,

Description of the "Vernon's" Engine and Machinery.

during the action of the steam, so much as to propel the vessel by its own force, at the rate of six or seven miles an hour. The engine being stopped and the wheels disconnected by the apparatus referred to, the way of the vessel is not impeded by their trailing in the water as fixtures, for they turn freely round, and thus offer little or no resistance to the ship's progress. During the voyage of the "Vernon," the wheels were frequently disengaged while the vessel was going about or tacking, the contrivance, which fully answered the purpose, being the very simple one of attaching a moveable head to the paddle-shaft crank, which being turned a quarter of a circle liberates the crank-pin, and enables the wheel to revolve freely in whichever direction the motion of the water impels it.

The boiler, cylinder, and steam-pipes were enveloped throughout with felt clothing doubled, and covered with deal staves 2 inches thick, hooped with iron and painted. This non-conducting medium was found to be so effectual, that the thermometer only rose 3 degrees on being hung on the covering of the boiler while the engine was at work, the minimum heat underneath the covering being probably at 220°. The advantages derived from this plan were, that not only was greater safety insured to the ship, as well as a considerable saving effected in the expenditure of fuel, but also the personal comfort of the passengers in the tropical latitudes was greatly increased by the slight extent to which the employment of steam affected the temperature of the vessel. This system which was first applied to the boilers, &c., of Her Majesty's ship "Gorgon," is now very generally adopted throughout the Navy.

First voyage of the "Vernon."

In June, 1839, the "Vernon" having on board 900 tons of cargo, and 60 tons of coals, with a draught of 17 feet in the river, (16½ feet at sea,) steamed down to the anchorage at Gravesend from the East India Docks, in three hours. During the last three quarters of an hour, she made way against the tide unassisted by her sails, at the rate of five knots an hour, equal to five and three-quarters statute miles. A speed like this attained by the employment of a steam power of one horse to thirty two tons, astonished all on board, and whilst it exceeded the expectations that had been entertained of the engine's performances, it afforded a convincing proof of the small amount of power requisite to obtain a low speed. The engine made twenty four strokes per minute, appearing to work easily and well, and giving no indications of the mishaps which afterwards befel the machinery. On the third day from her leaving the Docks she sailed from Gravesend and made the voyage to Portsmouth, partly under canvass and partly by the aid of steam, at a speed,

varying according to circumstances, from five to seven knots an hour. In the Channel off Brighton, the piston rod broke; this was replaced by a spare one, and the vessel finally started from Portsmouth during a calm, the steam being used for the space of eighteen hours, when, being off Dartmouth, the second piston rod snapped at the same part where the first had failed, affording however no means of accounting for the accident or of preventing its recurrence, the rod itself being quite sound and without a flaw at the fracture, and the other parts of the machinery standing remarkably well. Under these circumstances the "Vernon" proceeded on her voyage, making use of her sails only, until she reached the Line, where calms generally prevail. The engineer then fitted his third and last piston rod, and the engine was again brought into action, the hopes of all on board being now directed towards this means of avoiding the delays of from six to eight days that are usually experienced by vessels in the calm latitudes. These hopes were however speedily disappointed by the fracture of the third piston rod at the same place in which the two former ones had given way, namely inside the cross head, against the shoulder. The vessel being thus thrown entirely on her sailing resources, the wheels were unshipped and the Cape was made in eighteen days from the time of the last accident.

Accidents to the Engine.

Upon examining the machinery at the Cape, it was discovered that an original defect existed in one of the paddle shafts, the main bearing of which being split into two equal portions, threw the whole strain of the engine at each stroke upon one arm of the cross head, and thus the piston rods were snapped in succession close off at that part.

The necessary repairs being done to the shaft by the engineer, with the assistance of a Dutch boor, the "Vernon" again proceeded on her voyage to Calcutta. After she had passed Madagascar, light winds alternating with calms, prevailed during the remainder of her course through the Indian Ocean and up the Bay of Bengal, and the engine being now brought into requisition, performed its work in a manner that proved most essentially serviceable to the ship. During eight days and nights the engine was kept in constant work, the vessel averaging $4\frac{1}{4}$ knots an hour (5 statute miles) when steady, and 4 miles when rolling under the swell of the sea. She steamedup the Hooghley against the current, passing the ships at anchor, and finally anchored off Chandpal Ghaut, in the month of November, 1839, having made the passage in ninety-five days.

Completion of the "Vernon's" voyage and speed obtained.

The news of the arrival of the "Vernon," and of the peculiar qualifications

afforded by her fittings for making a quick passage, soon spread throughout the presidency, and applications for cabins and for freight became so numerous that the agents augmented the prices of the former one-fifth, and the freight of cargo 1*l.* per ton, at which rate the whole accommodation of the vessel was speedily taken up. It must be remarked that the East Indian public fully appreciate the high value of steam, being aware of the tedium and *ennui* to which they are exposed by the calms often experienced in the voyage to and from India; nor does the most frightful gale of wind convey to the seaman half the horrors which he attaches to the idea of being becalmed on the Line for a week or a fortnight beneath a vertical sun.

Return Voyage of the "Vernon."

The "Vernon" raised her anchor on the 28th February, 1840, and steamed down the river on her passage homewards amidst the acclamations of thousands of spectators whose curiosity had been excited by her performances. The "Windsor," another ship belonging to the same owner, and of about the same tonnage, had sailed exactly three weeks previously. In the Bay of Bengal the "Vernon" encountered a constant calm, and the engine was in consequence unremittingly worked during the space of fifteen days. On the 17th April she made the Cape and steamed into Table Bay, coming to an anchor off the town during the night to the astonishment of the natives, no vessel having up to that moment ever ventured, owing to the dangerous nature of the ground, either to enter or to quit the anchorage after sunset. Vessels in consequence of this circumstance frequently lie becalmed under the table land during three or four days, while, by the aid of her steam, the "Vernon" accomplished this in six hours. Having stayed a week at the Cape the vessel started again on the 23rd of April, under steam, which she used occasionally during the ten subsequent days. On the 6th May, St. Helena was left astern, and finally Spithead was made on the 4th June, the passage from the Cape having been thus made in the unprecedentedly short period of forty-two days between the raising and the casting of the anchor; a passage which for quickness exceeds all the performances of the largest steamers that have gone round the Cape, the shortest of those passages having been fifty-two, and the longest sixty-two days.

Duration of Voyage and speed obtained.

The "Vernon" steamed up the Channel, passing the "Windsor" lying in the Downs waiting for a tug, thus gaining upon that vessel twenty-one days. She arrived at Blackwall under steam on the 10th June, the "Windsor" reaching the same place on the 12th, whereby a further

gain of two days was added to the former. The whole distance between Spithead and Calcutta was accomplished, by the alternate agency of steam and wind, in the short period of ninety-five days, and deducting seven days for stoppage at the Cape, the duration of the passage is reduced to eighty-eight days, being one of the shortest passages on record.

Great praise must be awarded to Captain Denny, her commander, whose valuable assistance on all occasions to the engineer, and whose enthusiasm and courage, which never once abated, greatly promoted the success of this experiment; although the early difficulties of the outward voyage were of a nature to dishearten him, and to lessen his confidence in the powers of steam.

The "Earl of Hardwicke" fitted with an Auxiliary Engine, and starts for India.

The success which attended the voyage above described induced the owner of the "Vernon," to equip the "Earl of Hardwicke," a sister vessel, with an engine of the same power. The experience acquired during the experimental voyage however led the engineers to introduce some modifications into the machinery of both the engines.

The "Earl of Hardwicke" started on her outward voyage on the 20th August, and made the Sand-heads on the 8th December. The passage was certainly a long one, but the autumnal season of 1840 was characterised by very tedious passages. The steam was used during three hundred and sixty-four hours at various intervals of the voyage out, and the number of miles gained thereby in the calm latitudes, and where without it the vessel might have been delayed for days together, amounted to nine hundred and forty-six. Captain Hemming, in a communication addressed to Messrs. Seaward, accompanying an extract from his log, wherein the exact work done by the auxiliary steam is noted, states that the performances of the engine were satisfactory.

Result of Voyage.

"In a calm with perfectly smooth water," says Captain Hemming, "it will move the ship $4\frac{1}{2}$ knots an hour. As the ship gathers way from a breeze the engine's propelling power increases in proportion. When the ship will sail 5 knots with canvass the engine will increase her rate to 7 knots, and when sailing close-hauled the engine makes the ship hold a much better wind. We have had no accident with it. The consumption of coals is at the rate of $3\frac{1}{4}$ tons in 24 hours. I did not reef the paddles any more as the ship lightened daily, and they are not too deep for a sea-going steamer; but I have never been able to get more than 22 revolutions, even when the ship has been going through the water 8 knots by canvass, at which rate the piston could have little resistance

to overcome. I very strongly approve of and recommend the simple and expeditious way of disconnecting the paddles, which we had recourse to so constantly during the passage."

Second Voyage of the "Vernon."

The "Vernon" steamed down the river and through the channel to Portsmouth, in the month following after the departure of the "Hardwicke," leaving England finally on the 19th September, 1840, and arriving at the Sand-heads on Christmas-day, thereby gaining 13 days upon her predecessor.

The log of the "Vernon" had not yet arrived when these observations were penned, but a letter from her commander, Captain Denny, dated Calcutta, January 16, 1841, has been received, wherein he speaks in the most favourable terms of the effects of the auxiliary steam on board his ship, as having enabled him to gain six weeks upon several vessels which had sailed from England early in August. So favourably impressed was Captain Denny at the period when he wrote with the power of auxiliary steam, that he recommends it to be used to a larger extent than that to which it is carried in the "Vernon." "The advantage," he says, "of having double the steam power would be, first, that it would take but little more room, or no more if occupying the two decks, and only about half as much more coals; no more engineers, and it would take you more quickly through the calm spaces, and render ships more independent of steam-hire in going up rivers. The circumstance of getting more rapidly through calms would reduce the expenditure of coals to our present quantity. My passengers wish it much, and all speak well of the 'Vernon.'" The consumption of fuel on board these two vessels was 90 tons each during the outward trip.

Result of Voyage, and Comparison with the "India" Steam ship.

The "India" steam ship of 800 tons burthen, with engines of 300 horses-power, started from Portsmouth on the 7th of September, and had not yet arrived when Captain Denny's letter above referred to was despatched from Calcutta. It is since known, however, that this vessel reached the Sand-heads in one hundred and thirty-two days. All that remains now to be done in the way of further illustration of the views laid down in the preceding pages is to offer a concise statement of the comparative expenses of a vessel navigated in the manner described by auxiliary steam, and of a vessel which like the "India" depends wholly upon her engines for her progress; or if not wholly, is at least constructed in such a manner that her engines and their appurtenances and stock of fuel form by far the most important item of her contents and expenditure.

Comparison of Expenses of Steam as Sole and as Auxiliary Moving power.

The expenses attendant upon the employment of auxiliary steam in the "Vernon" were as follows:—

	£.	s.	d.
Interest of capital sunk in the purchase of engines, 1800*l.* at 5 per cent., 4 months	30	0	0
Wear and tear of ditto at 10 per cent., 4 months	60	0	0
Ninety tons of fuel at 1*l.*	90	0	0
Loss of stowage-room, 30 horses-engine at 1 ton per horse. 30 tons at 3*l.* per ton	90	0	0
Loss of stowage room on 90 tons of coals at 3*l.*	270	0	0
Wages of 1 engineer at 10*l.* per month, 4 months	40	0	0
Wages of 1 stoker at 5*l.* per month, 4 months	20	0	0
Total	£600	0	0

The presumed expenses attendant upon the "India" for the same period, calculated at the same rate of expenditure as the above, would be as follows:—

	£.	s.	d.
Interest upon the cost of the engines, 15,000*l.* at 5 per cent., for 4 months	250	0	0
Wear and tear, 10 per cent., 4 months	500	0	0
Coals, say 55 days at 25 tons a-day, upon the supposition that she steamed half the time occupied in her passage: 1375 tons at 1*l.*	1,375	0	0
Loss of stowage of engines, 300 horses, at 1 ton per horse: 300 tons at 3*l.*	900	0	0
Loss of stowage in fuel: 300 tons at 3*l.*	900	0	0
Wages of 4 engineers, averaging 9*l.* per month	144	0	0
Wages of 14 stokers and coal-trimmers at 4*l.*	224	0	0
	£4,293	0	0
Deduct cost of auxiliary steam in "Vernon"	600	0	0
	£3,693	0	0

Showing an excess of that amount of expenditure above the cost of employing a small steam power in a vessel, which was thereby enabled to gain at least 35 days over the more costly apparatus of the "India."

These remarks and the comparison above instituted are by no means in-

tended to disparage the "India" or the engines on board of her. The appositeness of the unintentional rivalry of the two ships at the moment when a new and interesting experiment was in a course of trial, has alone induced the writer to give the results as above stated, merely with the view of strengthening his own arguments in support of the system of navigation which he is endeavouring to introduce. It is satisfactory to state that other vessels engaged in the Indian trade have been equipped with auxiliary steam machinery, the advantages of which are beginning to be generally understood.

In conclusion it may be observed that in a preliminary essay, such as the present, it was not deemed necessary, in order to meet the views of practical men, to enter into any recondite calculations for the purpose of justifying the opinions or elaborating the ideas intimated above; the writer preferred having recourse to a plain statement of facts supported by such examples as the abundance of steam-vessels of all sizes and powers now afloat so amply afforded, a careful examination of which will enable any person to draw a right conclusion for himself.

Theoretical speculations upon such a subject, however suited to the philosopher's study, are of little value to the practical engineer or shipbuilder. To quote the language of the late revered president:—"What we want for this society is the development of that knowledge which is founded upon practical experience;" and judging of the final success of the experiment by the results at present obtained, it will probably be admitted that the expectation is not too sanguine which looks forward to the general and speedy adoption of auxiliary steam throughout the whole commercial navy, when, and when only the advantages of steam navigation will have received their full and true development.

SAMUEL SEAWARD.

Canal Iron Works, Limehouse, January, 1841.

APPENDIX I.

Since the foregoing Memoir was written, the Engines of the Vernon and the Hardwicke have been removed from their respective ships, and thus the more general application of auxiliary steam power to sailing vessels has for the present been suspended; a circumstance greatly to be regretted, as preventing the more complete development of a plan, judged to be of the highest importance to navigation.

Several circumstances conspired to cause the removal of these engines: the Hardwicke did not make a very short voyage, either out or home, and although the captain of her admits that by the use of auxiliary steam the duration of the voyages was considerably abridged, yet being himself part owner of the ship, and finding on his arrival at Calcutta, that freight was at a high rate, he calculated that could he have availed himself of the space occupied by the engine and coals, his pecuniary advantage would have been greater than that gained by the saving of time, through the use of steam power; he therefore availed himself of an opportunity which offered at Calcutta, to sell his engine for the sum it had cost, and it is now on its way back to India.

The Vernon, on the contrary, made two very short voyages, both out and home, and the interesting letters of her commander, Captain Denny, (than whom no one could be more capable of judging the advantages of this new application of steam power.) show how perfectly he was satisfied with it. This very success, however, has led him to become an advocate for an increase of power, from 30 to 50 horses, under the impression that there would be a proportionate gain in point of time, without any material increase in the consumption of fuel. It was, however, determined that the Vernon should proceed on her third voyage, without any change of engine. About the time she was preparing to start, a contract was entered into by Mr. Green, (her owner,) for the immediate conveyance of 1000 soldiers to India, and the whole available space in the three ships he had at home being required for that purpose, it became necessary to take the engine and machinery out of the Vernon which was one of them, and she sailed from Gravesend in August last, with 300 soldiers on board.

It rarely happens that an invention or a novel application of a system presents pecuniary advantage on its first essay, but notwithstanding the circumstance of the engines being taken out of these ships, before the benefits which it is confidently believed must result from the adoption of auxiliary steam power could be fully demonstrated, still much has been effected. The success of the Vernon on both her voyages must be nearly conclusive as regards the advantages of the plan, of which her owner is so fully impressed, that far from abandoning it, he is preparing to adopt it, still more extensively, in the ships he is at the present moment constructing for the East India trade. The prejudices of seamen are almost proverbial, so that some opposition was to be expected from them, not only from the novelty of the system, but that it added somewhat to their labour, as they were occasionally called upon to hoist up coals from the hold, as well as to aid the stokers; occupations which they were both unused to, and averse from: neither had they any interest in diminishing the duration of the voyage, as the amount of their wages was curtailed by the number of days gained. The officers, too, were somewhat opposed to the plan, from their ignorance of the nature of the machinery, and that it occupied some of the space which might have been devoted to increasing the comfort of their cabins; the commander and the passengers, on the contrary, fully appreciated all the advantages of the system, as is evident by the letter addressed to Captain Denny by the passengers, and which letter appeared in the public prints of the day.

It is certain that further essays will be made, and if they shall be aided by the commander of the ship, whose personal interests ought not to be injured by the plan, the ship owner will soon see the advantages of it, and gladly avail himself of a plan by which a long sea voyage may be considerably diminished, and the comfort of the passengers promoted, with increased safety to the cargo. It is, therefore, confidently hoped that the application of auxiliary steam to sailing vessels will speedily be more perfectly developed to the advantage of marine enterprise.

Canal Iron Works, Limehouse, November 10, 1841. SAMUEL SEAWARD.

APPENDIX II.

Auxiliary Steam to Vessels of War.

THE previous description of auxiliary steam refers only to merchant vessels. But it must be obvious to all naval and scientific men, that it is equally applicable to vessels of war; in fact, this has always been one of the favourite objects of the writer of the preceding essay, as he feels confident that the time is not far distant when no British line-of-battle ship will be considered completely equipped without this useful auxiliary; from their size, and from the fact of their carrying no cargo in the hold, more ample space for placing the machinery and stowage of the fuel is to be found on board these ships than on board merchant vessels, added to which, from the superiority of their forms for sailing, a less proportionate power will produce the velocity required.

There can be little doubt that in all the operations of a naval engagement, the possession of a locomotive power, by which a line-of-battle ship could move four or five knots per hour in a calm, would give her an immense advantage over her opponents; and, it should be borne in mind that most of our great naval engagements have been fought in a calm, or with such a light breeze that the firing of the guns caused such a stillness in the air as to leave the ships nearly helpless; it is then that they foul one another and get into confusion, and, as the smoke of the firing occasionally subsides, it is not unusual to find that they have been firing into ships belonging to their own flag instead of those of the enemy; it is at such times that the full value of auxiliary steam would be duly appreciated; ships having steam power would then take up a position by which all or most of the fire of the enemy would be lost or unnecessarily expended, whilst they would continue to rake the enemy till the decks would be completely stripped of every living soul; the heaviest three-decker could not withstand the well-directed fire of a 74-gun ship, supposing the latter to be able to choose her own position.

The noble class of ships now introduced into our Navy by that talented constructor Sir William Symonds, affords a most favourable opportunity of trying auxiliary power: their beautiful lines, their seagoing qualities, and great speed under canvass afford the best assurance that a very small power would be sufficient to propel them at the rate required; ample room can also be found on board these ships to place the necessary power, without interfering with a single gun or any of the usual stowage for ammunition.

Feeling impressed with the advantage which auxiliary steam would give to our frigates and line-of-battle ships a letter was addressed by the Messrs. Seaward to Sir William Symonds, the Surveyor of the Navy, upon the subject in the month of October, 1839, and by him it was strongly recommended to the notice of the Lords of the Admiralty; the subject was by them most favourably received: but as their Lordships at that time had already commenced building the formidable fleet of war steamers now nearly completed, and as there was no immediate apprehension of a war, the project remains in abeyance.

In that letter it was proposed to equip the "Vanguard," 80-gun ship, with an engine of 60 horses-power, the engine to be so arranged that it could also work the pumps in the event of a considerable leak, as well as be used for heaving up the anchor.

The drawings sent with the letter showed the "Vanguard" in section, with the space on the lower deck occupied by the engine, the paddle-wheels, boiler, &c.; and the area required was exactly the size of the sail-room, and in the same position, so that the most vulnerable portions of the engine and boiler were under the water-line.

SAMUEL SEAWARD.

Canal Iron Works, Limehouse, June 30, 1841.

APPENDIX III.

Log of the "Earl of Hardwicke" Indiaman.

VOYAGE of the Ship "EARL OF HARDWICKE" from London to Calcutta, with Auxiliary Steam.

1840.	Latitude.	Longitude.	Whole Distance Sailed.	Hours Steaming.	Distance Steamed.	Winds.	Remarks, &c.
	North. ° ′	West. ° ′					
August 23	48 55	5 31	Reckoned from noon to noon.	Reckoned from noon to noon.	Reckoned from noon to noon.	N.W. Calm.	On Thursday, 20th August, sailed from St. Helen's Roads, with a light westerly wind; beat down Channel, and on Sunday morning, the 23rd, took our departure from the Lizard, at noon, latitude and longitude as per margin; the wind declining to a calm, attempted to steam but found the engine would not work; employed overhauling it, re-stuffed the air-pump bucket, and at 10.30 began steaming; made 14 revolutions per minute, and propelled the ship three knots per hour.
,, 24	48 01	6 13	61	13½	47	S.E.	Light airs from S.E. continued; steaming increased to 16 revolutions.
,, 25	46 27	7 00	100	24	60	N.W. S.E. to	A.M. Light breezes at noon; found from the most accurate estimate the engine to have propelled the ship 60 miles in 24 hours. The following appears at present an accurate estimate of its power:—In a calm with smooth water, it will move the ship 4 knots; as the ship gathers way from a breeze the engine's propelling power decreases in proportion: when the ship will sail 5 knots with canvass, the engine will increase her rate to 7 knots, and when sailing close hauled, the engine makes the ship hold a much better wind. Noon. Light breezes from northward, the ship sailing 6 knots; let off the steam, and disconnected the paddles. Sunset, inclining to calm. At 8, the ship sailing only 1 knot, began steaming, and went 4 and 5 knots, making 16 to 18 revolutions. Light airs, 16 revolutions. Noon, an increasing breeze: in disconnecting the engine, strained the starboard radius and side rods after the larboard were disconnected from the ship, having too much way; if the ship is going fast, it is necessary to heave-to to disconnect, which is done in a few minutes.
,, 26	45 11	8 27	99	16	52	E.	P.M. Steady breezes, and cloudy; lightning to the southward: barometer steady 30, 18. It is remarkable that we should have had 53 hours' calms, and steamed 159 miles across the Bay of Biscay.
,, 27	42 50	11 43	200	..	..	E. to N.N.E.	Steady breezes and cloudy. Engineer repairing the radius and side rods; cleaned the engine throughout.
,, 28	40 56	13 17	133	..	..	N.E.	Steady breezes and cloudy: under all sail. Sunset, a decreasing breeze.
,, 29	39 38	14 12	91	4½	18	N.E. Calm.	Light breezes and cloudy, with light showers; showed colours to a line-of-battle ship with a rear-admiral of the red flag, supposed the "Donegal." Noon, light airs, nearly calm; sailing 1 knot. At 12.30 commenced steaming, 4½ to 5 knots. At 5 a breeze from N.E. disconnected. N.B. Blew off generally every two hours, or whenever the "Saline Detector," a very useful instrument, indicated it was necessary.
,, 30	37 59	15 10	111	..	..	N.E.	Moderate and fine under all sail.
,, 31	35 38	16 30	150	..	..	N.E.	Unsteady breezes and fine weather.
Sept. 1	33 01	17 16	163	..	..	N.E.	A.M. Steady breezes and fine weather. Noon, saw Madeira S. ¼ W. 4 leagues the West Point. Passed it and caught the N.E. trade.
	Carried	forward.	1108	58	177		

VOYAGE of the Ship "EARL OF HARDWICKE"—*continued.*

1840.	Latitude.	Longitude.	Whole Distance Sailed.	Hours Steaming.	Distance Steamed.	Winds.	Remarks, &c.
	Brought forward. North. ° ′	West. ° ′	1108	58	177		
Sept. 7	18 52	21 26	883	3	7½	N.E. Calm.	The N.E. trade, which commenced with a fine fresh breeze, soon became light and unsteady; continued running with it under all sail until the morning of the 7th instant, when it appeared decreasing towards a calm. At 9 A.M. commenced steaming; made 17 revolutions. Noon calm; position in the margin; the engine propelling the ship 4½ knots in a perfect calm.
,, 8	16 41	21 38	132	24	72	E.N.E.	A.M. Light easterly airs and calms, making 17 to 20 revolutions.
,, 9	14 57	21 33	104	24	70	N.E. Calm.	A.M. Light airs and calms, a swell checking the engine's power. Sunset, a breeze from N.W. Increasing, disconnected, and left off steam.
,, 10	12 44	21 44	134	10	30	North. East. S.S.E.	A.M. Moderate breezes and fine clear weather. Noon, ditto, a southerly swell; 8 P.M., inclining to calm: commenced steaming, 19 revolutions, light N.W. airs assisting.
,, 11	10 51	22 09	115	10	35	N.E. East. Calm. S.S.E.	A.M. Light airs and calms, with a southerly swell; engine making 16 to 19 revolutions. At 6.20 a breeze from east; disconnected and eased the fires. Noon, light breezes from the southward, and cloudy. At 8 began steaming. At 9 a heavy squall from S.E. short sail, hove-to, and disconnected; carried away the fore-sheet, which got foul of the paddles. Under double reefs; fresh breezes.
,, 12	10 22	21 58	32	3½	8½	S. by W. W.S. Calm. N.W.	A.M. Fresh breezes and cloudy, with rain and squalls. At 6, moderate, all sail set. At 9.30 light breezes; began steaming. Noon, calm, a confused southerly swell checking the engine's power, making only 13 revolutions, and propelling the ship 2½ knots per hour against the swell. P.M. Sharp squalls, 6.30, hove to and disconnected.
,, 13	9 03	20 55	101	10	25	N.W. W.S.W.	Light airs and squally; a heavy southerly swell. At 10.30 began steaming. Noon, squally with rain, the ship running 8½ knots; the engine making 21 revolutions. Sunset more steady; settled looking weather, and moderate breezes, but a heavy southerly swell.
,, 14	7 37	19 12	137	24	50	S.W.	Light steady breezes from S.W., ship close hauled, engine assisting and making 16 to 18 revolutions. Noon moderate and fine; observed two of the lee paddles broken at the outer arm. P.M., moderate breezes, varying in strength, and cloudy; the engine making from 18 to 22 revolutions, and jerking with an uneven motion, on account of the two broken paddles and the swell.
,, 15	5 36	17 30	161	15½	30	S.W.W.	A.M. Squally, with rain, the ship running 8 knots. At 3.30 hove-to, disconnected, and let off the steam. Noon, squally, with showers; unshipped half the paddles. Steady breezes, all sail set.
,, 29	South. 18 56	17 14	2082	..	..	..	From 15th to the 22nd September, encountered S.S.W. and southerly winds. Crossed the Line on the latter day in long. 12° W., when the southerly winds had gradually hauled round to the S.E. trade, which proved a moderate one, and set the ship to only 18° 44′ W. long. On 29th September, at noon, at the place noted in the margin, towards evening, the wind, which was from N.E., had gradually decreased, inclining to calm. At 11.30 began steaming, and steered S. by E.
	Carried forward.		4989	182	505		

VOYAGE of the Ship "EARL OF HARDWICKE"—*continued.*

1840.	Latitude.	Longitude.	Whole Distance Sailed.	Hours Steaming.	Distance Steamed.	Winds.	Remarks, &c.
	Brought forward. South. ° ′	East. ° ′	4989	182	505		
Sept. 30	20 21	16 18	100	12½ 1	31 2	Calm. N.W.	A.M., light airs; a heavy swell on the beam. At 2 a perfect calm, engine making 17 revolutions, and propelling the ship 3½ knots, the ship rolling deep. Noon, a breeze, increasing from northward and clear. 1 P.M. disconnected, and going 6 knots.
October 10	30 31	5 07	1008	..	..	Easterly. Variable. S. by E.	Unsteady, variable; contrary winds prevailed until the 10th October, during no period of which steam could be used. Noon, at the place noted in the margin P.M., light airs, declining to a calm. At 6, began steaming, a heavy swell on the starboard beam, making the ship roll deep, the paddles alternately immersed and out of the water, making 15 revolutions, and propelling the ship 2¾ knots. At 8, a light air from the southward, furled all sails, and continued to steam 2¾ knots, with 15 revolutions.
,, 11	31 32	4 43	65	18	49	Variable. Calm. East.	A.M., calm and fine weather; a swell on the beam as before; steaming 2¾ knots. Noon, calms and variable airs; all sail. At 6 P.M. calm; furled all sails. 9.30, a breeze from the east, disconnected and made sail.
,, 12	33 02	3 30	109	11	30	East. North. Calm.	A.M., light breezes from the eastward decreasing. At 4, began steaming. At 6.30, a light breeze, disconnected. P.M., decreasing breezes. At 11.20, began steaming; a swell on the beam as before.
,, 13	34 35	2 39	103	6	17	N. to N.W.	A.M. Light airs and cloudy, 17 revolutions. At 6.30, a light breeze from north increasing, disconnected. Noon, a steady breeze, and cloudy, with rain. Having now reached those high southern latitudes where calms are unusual, unshipped the whole of the paddles and the funnel.
Nov. 10	27 59	87 23	4799	..	..	N.E. to N.W.	From the 13th October, employed running to the eastward, but found winds more unfavourable than generally prevail in the parallels of 38 and 40 degrees south latitude; after which steered to the northward to reach the S.E. trade, on the 10th of November, position in the margin, where calms are occasionally encountered, having in the interval repacked the cylinders, cleaned the boilers throughout, and overhauled the engine; shipped the funnel and upper paddles to be in readiness. P.M., the breeze decreasing and inclining to calm. At 6.30, commenced steaming, a light air from N.W. Engine making 14 revolutions, and propelling the ship 2½ knots, and 2¾, furled all sails, and steered N.N.E. A westerly swell making the ship roll.
,, 11	27 10	87 45	53	17	40	N.E. N.W. S.E.	A.M., light airs continuing from the northward; 14 and 15 revolutions, 2½ knots. At 6, a breeze from N.E. made all sail, and steered N.N.W., and kept the engine going, increased to 4½ knots. P.M. Light airs and calms, a westerly swell continuing. At 10, air increasing, breeze from S.E. disconnected and left off the steam, having caught the S.E. trade.
,, 12	25 33	87 48	98	10	25	Easterly.	A.M. Unsteady breezes from east and cloudy. At daylight unshipped half the paddles. Noon, steady breezes and fine weather.
,, 23	North. 2 02	93 46	1793	..	..	N.E. S.E. Calm.	From the 12th to the 23rd instant, not having calms, the engine was not required at noon, the ship's position in the margin. P.M. Light breezes decreasing; employed getting the engine ready, the ship having lightened considerably, drawing about 15½ bodily; let out the reefs of the paddles. Mid. calm, commenced steaming.
	Carried forward.		13,116	257	699		

VOYAGE of the Ship "EARL OF HARDWICKE"—*continued.*

1840.	Latitude.	Longitude.	Whole Distance Sailed.	Hours Steaming.	Distance Steamed.	Winds.	Remarks, &c.
	Brought forward. South. ° ′	East. ° ′	13,116	257	699		
Nov. 24	3 35	93 19	93	12	36	N.E. Calm. S.W.	Light airs from N.E.N. and cloudy weather. Steered N.W. by N., constant sheet lightning; engine making 14 to 17 revolutions, and propelling the ship 3 and 3½ knots. P.M., light variable airs altered the course occasionally, to benefit by the light airs in steering to the northward, and kept the engine going. At 8, west breezes and cloudy.
,, 25	5 13	93 01	100	17	37	Westerly. Calm. Westerly. East.	A.M. Squally with rain, the feed-pipe having broke had been parcelled, but being leaky, did not supply the boiler sufficiently. No steam generated, disconnected to repair it, and refilled the boiler with the force-pump, the hot well became too hot to condense; took off the apron, and employed cooling it with cold water. At 7, commenced steaming. At 10, a light breeze ahead, furled sails; the engine making 14 and 15 revolutions, and propelling the ship only 1¼. At 11, the feed-pipe again leaky; stopped the engine to repair. Noon calm, the pipe being repaired at 12.30 began steaming. At 1, a light air from westward, made all sail, 16 revolutions. Midnight, light variable airs with rain.
,, 26	7 14	93 08	122	18	38	East. E.N.E.	A.M. Light breezes from the eastward increasing. At 6 hove to, disconnected and unshipped half the paddles. Mem.—As the engine had propelled the ship more slowly than before, supposed the paddles to be too deep; reefed them as before; engineer repairing the feed-pipe. Noon, light breezes from the eastward; saw the Great Nicobar, east 10 leagues. P.M., moderate breezes from eastward, and passing showers.
December 4	16 35	91 13	722	..	..	N.E. by N. N.E.	From the 26th November to the 4th December, encountered the usual N.E. monsoon wind, which had set in steadily. At noon, latitude and longitude as per margin. P.M., steady light breeze from N.E. decrease. Sunset, light breeze declining. At 11, ship going only 1 knot, began steaming, and increased her rate to 3, 4, and 5 knots; a light air assisting.
,, 5	18 10	By Lunar 90 04	102	13	40	N.E.	A.M. Light airs from N.E., engine making 16 revolutions. At 8, light breezes, stopped the engine, and found the ship sail 2½ knots. Gave her the steam, and found it increased her rate to 5½ knots. Noon, light breezes and fine clear weather, making 16 to 21 revolutions; a light breeze assisting.
,, 6	19 36	89 24	97	24	54	N.E. by E. to N. by E.	A.M. Light breezes and fine clear weather; wind varying from N.E. by E. to N. by E., employed working to the northward, assisted by the engine. Noon, light breezes from N.N.E. disconnected. P.M., the wind decreasing, recommenced steaming.
,, 7	20 33	88 36	74	23	42	N.E. by N. N. by E. N. by W.	A.M., light breezes from N.E. by N., all sail set, steaming with full power, and making 20 revolutions. At 8, the wind more northerly, disconnected, having very few coals remaining. P.M., light airs from N.N.E., employed turning to windward.
,, 8	21 04	88 14	40	..	..	N.N.E.	A.M., moderate breezes from N.N.E. At 2, saw the pilot vessel. At 8, Mr. Cooper, master pilot, came on board, and took charge of the ship. At 9.30, came to near the Hoogly Light to stop tide.
		Total.	14,466	364	946		

APPENDIX IV.

The accompanying Table of the Velocity of Steam Ships with Engines of various Powers is the result of numerous experiments with steam vessels ranging from the burthen of one thousand tons, down to that of sixty tons, under different circumstances of draught of water; but invariably in calm smooth water without either current or tide.

This Table fully corroborates the facts contained in the memoir, and the deductions drawn from them, while it is evidence of the small power requisite to propel a vessel of considerable magnitude at a moderate speed: it also shows that to gain a velocity which shall exceed ten miles per hour, it becomes necessary to augment the power of the engine in an immense degree.

The Table terminates in all cases with the velocity of 13 knots, equal to 15 statute miles per hour: this speed has not hitherto been exceeded, while in order to arrive at it, the whole tonnage of the vessel is occupied by the engine, leaving no room for cargo, and only space for the fuel necessary for about two days' consumption.

This immense speed has been attained by two or three vessels only, and it has been arrived at by adapting the vessel for speed alone—making every other quality of the ship subservient to this one object. The vessels running on the Thames are known to be the fastest in the kingdom: the most celebrated among them are the "Ruby," "Railway," "Blackwall," "Brunswick," and "Eclipse," the whole of which were built and had their engines constructed and fitted in London.

S. S.

1200
1150
1100
1050
1000
955
910
865
820
780
740
700
660
625
590
555
520
490
460
430
410
375
350
325
300
280
260
240
220
205
190
175
160
150
140
130
120
115
110
109
107
105
103
102
101
100

This side Line represents the Tonnage of the Steam Ship ranging from 100 tons to 1,200 tons.

XVIII.—*On the Percussive or Instantaneous Action of Steam and other Aëriform Fluids.*

By JOSIAH PARKES, M. Inst. C. E.

Read May 25, 1841.

In my last communication, "On the Action of Steam in Cornish Single Pumping Engines," it was shown, by the analysis of three examples, that the simple elastic force exerted throughout the stroke—as ascertained by the ratio of the volumes of water, and steam consumed—was insufficient to overcome the opposed resistance. The appearance of this deficiency of power, on the preceding well known, and usual manner of estimating it, led me to an examination of the particular circumstances under which the steam is employed in those engines; and I came to the conclusion that though steam, in a state of quiescence, or of equilibrium with surrounding matter, possesses only a certain pressure at given densities, it will produce, under the conditions of its application, in such engines, a peculiar and specific action—independent of that due to its simple elastic force—which action being imparted to the piston, the effect will necessarily be exhibited in the final result. This action I then denominated the steam's percussive or instantaneous force, as contradistinguished from the exertion of its simple elastic continuous force (see ante p. 268); under the impression that the term percussion would convey to practical men a definite idea of the instantaneous force to which the piston would be subjected, on the sudden opening of the communication between the boiler and the cylinder.

For proofs of such a force having operated, I then relied upon, and cited various phenomena attendant on the working of the engines; considering that the substantiation of the views advanced should rather depend on facts disclosed by such phenomena, than be derived from any extraneous data, or phenomena, however close the analogies might be. If reasoning alone could have been deemed sufficient to establish the soundness of these views, I might have relied upon the general law of physics, that all matter, in motion, whether solid, liquid, or aëriform, has a specific momentum; deducing, thence, that a body at rest,

like the piston of an engine which opposes the motion of the aëriform fluid steam, must be struck by a force, identical, in nature, with that denominated percussion, when communicated by liquids or solids; also, that the effect of impact by one body on another, must vary only in degree, not in character; and, then, it might have been conceded to me to attribute to this action a share in the effects obtained by a Cornish engine.

But this mode of disposing of the subject would not have been satisfactory either to other engineers or to myself. Neither the fact, nor the amount of the deficiency of the steam's simple elastic force, compared with the resistance, were previously established. The extent of the steam's expansive action—its elasticity at the termination of the stroke, relatively to the load of water, and to the absolute resistance—as well as other important practical questions—required explanation. All these questions were involved in the general one of the steam's action in a Cornish engine. In default of positive experimental determinations, I ventured to assume that the apparent deficiency of the steam's simple elastic force might be compensated by the force of percussion; thus, the former amount was used, as the measure of the latter, in the analysis. I considered that I was not departing from the strict rules of philosophical reasoning, by applying this single deduction; being unable to discover any other source, whence the deficiency of power could be supplied; and being of opinion that the steam's percussive or instantaneous action was equivalent to the effect then assigned to it. It must be remembered that the elastic force of the steam, as determined by the method of volumes —that the continuous action due to that elastic force, during the steam's contact with the piston—that the force of the resistance opposed throughout the stroke—are ascertained with sufficient precision; and that, in the absence of all other explanations of the means by which a portion of such resistance may be overcome, any cause which has a real existence may be considered to be a true cause. That such a cause does exist—whatever may be the amount to be referred to it—will, I think, appear from the facts which it is my present object to lay before the Institution, in further corroboration of the opinions previously advanced. These facts are derived from a number of experiments, which show, in the most direct manner, the existence and great effect of percussive or instantaneous action in steam; and furnish an approximation towards an estimate of its amount at given elasticities. I shall also adduce some phenomena of the same nature from other aëriform fluids. [Note A.]

Experiments with the Indicator.

The results to which I shall first allude were obtained while engaged, professionally, in testing with the indicator the power of different engines. The diagram drawn by this instrument will, under certain conditions of structure, and use, afford tolerably correct transcripts of the steam's method of action, and of its force upon the piston of an engine. Forty-one diagrams were taken from four engines by different indicators; they supply examples of the steam's percussive or instantaneous action, through a range of absolute pressures varying between 6·5 lbs. and 34·7 lbs. per square inch.

These diagrams manifest that, in every instance, the indicator piston was driven upwards, higher than the steam's simple elastic force could have raised it. In many instances, a pressure is marked, greater than that which existed in the boiler: though, in every case, the action of the governor on the throttle valve caused the steam's elasticity in the cylinder to be considerably less than that in the boiler. When the steam valve, or the indicator cock is very suddenly opened, at high pressures, the motion of the indicator piston is so instantaneous, and its ascent so rapid, as scarcely to be followed by the eye; but, the pencil transcribes the effect on the diagram, and if the finger be held within the range of the extremity of the piston rod, above the true height to which the steam's simple pressure would raise it, it is powerfully struck. This phenomenon has occurred in all these instances during the regular working of the engines; that is, on each fresh admission of steam; and it takes place to a diminished extent, the more gradually the steam is admitted. [Note B.]

Experiment on the Mercurial Column.

I had an opportunity, whilst conducting some experiments on the newly invented Disc engine, of ascertaining on a cistern gauge, the height to which mercury would be raised in a glass tube, by suddenly letting the full pressure of the steam in the boiler upon the surface of the mercury in the cistern. The pipe and cock connecting the boiler with the gauge were one inch in diameter. The cock was first gradually opened, and the mercury rose slowly in the tube, exhibiting the steam's pressure in the boiler to be 40 lbs. above the atmosphere; equal to 54·71 lbs. absolute elasticity. The cock was then shut, and the mercury subsided into the cistern from condensation. The cock was again opened as rapidly as possible, and the mercurial column instantly indicated 59 lbs. above the atmosphere, or 73·71 lbs. absolute force. The effect was produced instantaneously, and it required close attention by the observers to determine the precise height to which the mercury ascended in the tube. Other experiments were made in a similar

manner at pressures diminishing from 40 to 21 lbs. above the atmosphere, and, in every case, the steam's sudden impact elevated the mercury to a height so nearly corresponding with the ratio of the foregoing, that I have deemed it only necessary to cite one result in the following table. [Note C.]

Experiment with steam of 26 atmospheres' elasticity.

Shortly after my last Paper was read, Mr. A. M. Perkins informed me that he thought he could afford me an example of the effect of the steam's percussive or instantaneous action, at elasticities much higher than those in common use. I found at his manufactory a steam generator to supply a steam gun, capable of resisting a pressure of many atmospheres. It was connected with an indicator constructed similarly to the common steam engine instruments, as regards its cylinder and piston, but denoting the pressures by an index moving round a divided dial plate, like the spring balance. A two way cock formed a communication between the instrument and the steam in the boiler, or the atmosphere, so that the steam's pressure could be applied to the piston, and removed at will. Mr. Perkins justly concluded that if I was correct in ascribing a superior effect to the sudden and full opening of the admission valve, in a Cornish engine, the index of his dial would be carried beyond the degree indicative of the steam's simple elasticity, on suddenly opening the cock of the generator.

Such was the effect; but the velocity given to the index was so great as to render its motion quite invisible. The eye could only distinguish that the index, which previously stood at zero, now marked 26 atmospheres, which was the pressure in the generator; but, we ascertained that it had been driven round the dial as far as 36 atmospheres. This was determined, on repeated trials, by applying the finger, or a strip of lead, to the dial plate, and moving it till the extreme stroke of the index was only just perceptible. [Note D.]

Table 1.

The results of all the foregoing experiments are classified in the following Table, commencing at the lowest elasticities, being those obtained from steam below atmospheric pressure.

The pressures marked in column 3 are those of the simple elastic force of the steam which existed in the cylinders, at the commencement of the down stroke, as shown in figs. 1 and 2. [Note B.] Column 4 registers the highest pressures denoted by the indicators, or mercurial gauge, on the instant of the steam's admission. The term *momentum* adopted in the heading of column 4 may be objected to on some grounds; but after much consideration, it appears the best in common use to include all the action besides that due to simple pressure.

TABLE I—*continued.*

Number of Experiments.	Mean pressure in the Boilers.	Pressure due to the Steam's Simple Elasticity	Pressure due to Momentum.	Difference of Elasticity and Momentum.	Particulars of the Engines, and Remarks.
No.	lbs. per sq. in.	lbs. per sq. in.	lbs. per sq. in.	lbs. per sq. in.	
30		28·71	37·71	9·00	Double cylinder expansive engine by Mr. Edward Humphrys, at Parrott's flour-mill, Broad-street, Lambeth.
31		31·71	41·71	10·00	
32		32·21	40·21	8·00	Diagrams from small cylinder.
33		32·21	40·71	8·50	Diameter of cylinder 8 in. Length of stroke . 32 ,, 42 strokes per minute.
34		32·71	37·71	5·00	
35		32·71	44·71	12·00	
36		33·71	37·71	4·00	
37		33·71	43·71	10·00	
38		33·91	45·71	11·80	
39		34·21	47·71	13·50	
40		34·46	44·96	10·50	
41	37·71	34·71	44·21	9·50	
42	54·71	..	73·71	19·00	Experiment on a mercurial gauge at the Disc engine Company's Works, Birmingham.
43	382·46	..	529·56	147·10	Experiment on Mr. Perkins's generator and index, London.
1	2	3	4	5	

Although, as might be expected, the indicator diagrams—taken as they were from differect engines, and the valves of which were opened with greater or less rapidity—do not furnish a scale exhibiting an uniform increase of percussive or instantaneous action, for increasing pressures, yet the fact is demonstrated that this action took place in every instance; and that its intensity increases with the steam's elasticity.

Approximate ratio between the elastic and percussive force of Steam.

From these experiments an approximation may be made to the amount of the action due to the steam's momentum.

Assuming Mr. Perkins's dial plate to have been truly graduated, it results that, for each pound of the steam's simple pressure, (experiment 43,) an instantaneous action was exerted on the piston equal to 1·384 lbs. per sq. in. The ratio on the mercurial column, (experiment 42,) is as 1 to 1·347. Experiment 39, which exhibits the maximum effect obtained at the highest ranges of pressure taken by the indicator, gives a ratio of 1 to 1·394. Experiment 7

shows a ratio of 1 to 1·427. It is important to notice that whilst taking this diagram, and that in experiment 2, which gave a ratio of 1 to 1·381, the excentric rod was disengaged, and the valve *handed*, which occasioned a fuller and more instantaneous entrance of the steam into the cylinder, than could be effected by the excentric's motion in the other experiments on the same engine.

It must be observed that none of these experiments were made with the expectation of arriving at an exact determination of the abstract amount of percussive or instantaneous action. An apparatus for experimentally solving this problem would require to be constructed expressly for the purpose, for the reasons hereinafter assigned (page 422). In consequence, therefore, of the inadequacy of the instruments, I can only employ the maxima of the effects denoted, as the nearest approximation to the truth; for, the highest result marked at any one elasticity, may be at all times obtained, from steam of similar elasticity, by using proper means.

Table 2. The following table exhibits the greatest results in a scale descending from the highest to the lowest elasticities experimented upon:—

TABLE II.

Number of Experiments Table I.	Pressure due to the Steam's Simple Elasticity	Pressure due to Momentum.	Difference of Elasticity and Momentum.	Ratio of Elasticity and Momentum.
No.	lbs. per sq. in.	lbs. per sq. in.	lbs. per sq. in.	Ratio.
43	382·46	529·56	147·10	1 to 1·384
42	54·71	73·71	19·00	1 to 1·347
39	34·21	47·71	13·50	1 to 1·394
7	11·71	16·71	5·00	1 to 1·427
2	9·71	13·41	3·70	1 to 1·381
1	6·50	9·00	2·50	1 to 1·384
Means	83·21	115·01	31·80	1 to 1·386

The results of these experiments are in perfect accordance with the general principle that the increase of force, arising from the steam's momentum, is in the direct ratio of its elasticity.

That the increment of the percussive or instantaneous action should be in

simple proportion to the increment of the elastic force of steam, appears to be consistent with its nature, as a perfectly elastic, and, dynamically considered, perfectly imponderable body; for, the weight of steam operating in an engine cannot form any appreciable quantity of its mechanical effort or effect. The motion of steam is simply due to its elasticity; or, more properly speaking, to the repulsive forces which exist between the particles: the velocity of that motion, and, consequently, the momentum of the steam—since it may be considered imponderable—will also depend, simply, on the elasticity. When these repulsive forces are in equilibrio with each other, or are counterbalanced by the reaction of any containing surface, the steam is at rest; but, so soon as that equilibrium is disturbed, as by opening a passage, or removing part of the containing surface, motion ensues; unless steam, of the same elasticity, be on the other side; in which case, there will be no motion, since the elasticities will then counterpoise each other. The velocity of the steam will depend on the difference of the elasticities; and, that this difference may produce its full effect —or that the steam may have the greatest velocity—the opposing surface must be withdrawn, or the communication made, instantaneously. As this, however, is not, strictly speaking, practicable, the more quickly the communication can be effected, the greater will be the velocity and momentum of the steam issuing from the boiler into the cylinder, and the greater the percussive or instantaneous action on the piston. [Note E.]

Difference between a single-acting and crank engine, on the instant of the Steam's admission.

I will now proceed to apply the information furnished by these instrumental tests to illustrate, still further, the theory of the steam's action in Cornish single pumping engines.

It is apparent that, in all the experiments above cited, a greater or less amount of percussive or instantaneous action was transmitted to the piston of an engine. It is also seen that in two experiments, distinct from an engine, the sudden admission of steam upon the surface of mercury in the basin of a gauge, and upon a piston connected with an index, produced an effect upon the column of mercury, and upon the index, more than a third greater than the actual elastic force of the steam. The fact of the steam's percussive action is irrespective of the engine; but its dynamic use depends on the circumstances of an engine at the time of its application. All the indicator diagrams, referred to, show that the valves of the engines were so set, as to let the steam upon the pistons, when the engines were absolutely at rest; or, more correctly, when the piston was on the point of reaching the extent of its stroke. Such, or very nearly similar,

is the condition of all reciprocating, double-acting, rotative engines. At this instant of time, the connecting rod and crank are in the same line—in beam engines, vertical—so that no amount of force, then impressed on the piston, can act with any useful effect. On admitting steam to engines so circumstanced, no other result is produced than a sudden pressure, which is perfectly useless. The simple elastic force of the steam in the cylinder acts efficiently, as soon as the altered position of the crank, through the agency of the fly-wheel, permits the piston to move: but, no motion, no useful effect, is produced by the steam's percussive or instantaneous action; as the condition of the engine, at the time of its being exerted, prohibits motion. The piston is not free to move at the instant of the admission of the steam, and its initial percussive or instantaneous action is expended ineffectively on the parts of the machine.

The condition of single acting engines is entirely different. The beam and scales of a balance truly represent it. There is nothing to oppose the descent of the piston on the instant of applying any kind of force to it, which exceeds the amount of the inertia of the mass, and other resistances. Thus, the piston is prepared—it is in the best condition—to receive, and yield to the exertion of any preponderating effort upon its surface; there is no crank prohibiting motion, at that instant, as in rotative engines; whatever, therefore, is the amount of the steam's force, and, of whatever nature that force may be, the whole must tell upon the piston, and be accounted for in the effect. By applying the ratio just given, of the excess of force arising from the steam's momentum over that of its simple elasticity, to the examples of the Cornish engine examined in my last paper, it will be found that the effect then attributed to it, is less than the *maximum* which can be obtained from the pressures employed by the Cornish engineers. [Note F.]

Phenomenon illustrative of percussive action in Cornish engines.

If further proof were wanting of the great excess of the steam's initial force over its simple elasticity, as well as of the nature of that force, I think it will be found in the following phenomenon communicated to me by Mr. William West, the designer of the Fowey Consols engine. He stated to me that additional load had been laid upon that engine, since the period of the trial which gave the facts used in my former investigation; and that he was fearful the cylinder cover would give way, as he observed it to "breathe upwards," very sensibly, at each admission of steam. I requested him to measure very carefully the amount of this springing of the

cover, and to supply me with the particulars of its weight, and strength of material.

The cylinder is 80 inches diameter. The cover weighs 4 tons. Its bottom is flat, $2\frac{1}{2}$ inches thick, strengthened on the upper side with 16 ribs, radiating between the centre and circumference, and averaging $2\frac{3}{4}$ inches deep, by $1\frac{1}{2}$ inch thick.

Notwithstanding the massiveness and strength of this construction, it was ascertained that the cover is sprung upwards at the centre $\frac{5}{32}$ of an inch by steam of 49·71 lbs., and $\frac{8}{32}$, or $\frac{1}{4}$ of an inch, by steam of 61·71 lbs. per square inch, absolute pressure in the boiler. This effect is instantaneous on the steam's admission.

To complete this experiment, Mr. West also tried, at my instance, to what amount the cover would be sprung by maintaining steam, in a state of quiescence, between it and the piston, at the full pressure of the steam in the boiler. He found, as I anticipated, that no change of shape whatever was produced in the cover by steam under these circumstances. He brought down the piston to the bottom of the cylinder, and let the engine hang, for some time, on the spring beams; the steam valve being kept open to establish a perfect equilibrium between the steam in the boilers, and cylinder. Its elasticity was 54·71 lbs. per square inch. Mr. West observes: "These experiments have been made as accurately as possible; there have been many covers, of the same dimensions, broken in Cornwall by the steam's blow." [Note G.]

Additional phenomenon illustrative of percussive action in Cornish engines.

I will now consider another phenomenon, also supplied by the engine, and which seems to me to be equally demonstrative of the great effect of the percussive or instantaneous action.

In the section of my last paper (p. 280) explaining the action of the elastic cushion of steam which brings the engine to rest, I restricted myself to the observation that some gain might arise from it, as it is a quantity of steam recovered at the end of each stroke—a quantity, in fact, replaced at each reciprocation—and which by its ultimate expansion below its original pressure, produces useful effect. I did not enter, at that time, into a consideration of the influence which the greater or less volume of this cushion might have upon the economy of the engine.

The nature of the work done by a single acting engine, having no crank, or other mechanical means of regulating the length of a stroke, requires that the

equilibrium valve shall be closed in time to imprison, between the piston and cover, steam of a pressure sufficiently great to bring the engine to rest, and prevent the piston from striking the cover. The time of closing that valve thus partly regulates the length of the working stroke, and positively determines the volume of the cushion. But it appeared to me that the less this volume, the greater should be the effect of the percussive or instantaneous action of the steam; and consequently, the greater the economy produced—or, what is the same thing, that the greater the space into which the initial steam has to expand, on entering the cylinder, the percussive force, or instantaneous action on the piston would be diminished in a corresponding degree. The inference, therefore, which I felt disposed to draw was, that the more the volume of the cushion could be contracted, the greater would be the force of the steam's impact on the piston.

Several Cornish engineers have since confirmed the soundness of this view: they assure me that, when making special trials of their engines, they always exceed, and sometimes very considerably, the amount of duty done in the regular course of work; for, when attending to the engines personally, they allow the piston to ascend till it nearly touches the cover; but they dare not trust the regular attendants to work in this manner.

It must be observed that, in effecting this increase of performance, no change has been made in the amount of resistance to be overcome; nor in the engine, with the exception that, on the termination of the return stroke, the piston rests nearer the cover. No change has been made in the elastic force of the steam in the boiler, nor in the lift, nor duration of the opening of the steam valve: the density of the steam in the cushion is also unaltered; its volume only is diminished. The consequence of this apparently simple circumstance is, that the working stroke is accomplished with a less consumption of steam. The cause of the increased effect seems to me to receive its explanation from the increased intensity of the steam's impact on the piston; and, hence, the superior economy of a small cushion. [Note H.]

Oscillation of Mercury in gauges.

A complete explanation of certain phenomena, commonly noticed by attendants on steam engines, appears to me to be afforded by the foregoing observations and experiments. I refer to the excessive oscillation of the mercurial column in steam, and vacuum gauges. It is a frequent remark that when an engine is in action, the mercury rises higher, and

falls lower, than is due to the exertion of the steam's, or the atmosphere's simple elasticity.

On a steam valve being suddenly shut, a certain amount of reaction takes place upon every portion of the internal surface of the boiler, and upon every object exposed to its influence. Consequently, the mercurial column yields to its force, and is elevated to a height exceeding the true measure of the steam's simple elastic force. Several instances of the complete evacuation of mercury from these gauges have been communicated to me, when the column was superior to the simple pressure of the steam, or of the atmosphere. I will cite two, as sufficient for the illustration, though I could enumerate many other similar phenomena; but, as they all find their explanation in the principles before stated, it would be mere iteration to quote them.

Instance 1.—A safety valve being very suddenly closed, whilst the steam was blowing off, and the pressure of which did not exceed 3 lbs. above the atmosphere in the boiler, the whole of the mercury was expelled from a syphon gauge, though it contained sufficient to indicate truly 6 lbs pressure.

Instance 2.—The mercury of a syphon vacuum gauge was blown into the condenser of an engine, on suddenly opening a cock of communication, though it contained mercury enough for a perpendicular column of 33 inches. The vacuum in the condenser did not exceed 28 inches.

These incidents are not uncommon, and engine-men not unfrequently incur blame from their masters, by losing the mercury of their gauges; the masters asserting that the safety valves must have been overloaded to produce the effect. A case has been reported to me in which a valve was weighed, to decide a dispute of this kind, and the engineer was found to be correct in his assertion that the mercurial column exceeded the pressure on the safety valve, though the gauge was frequently evacuated of its contents. [Note I.]

Phenomena of the impact of one elastic fluid on another.

The illustrations hitherto adduced have been derived from, or considered with reference to, the cases of an elastic fluid being brought to act, suddenly, on a solid, or a liquid, but, in several of these cases, the percussive or instantaneous action is rendered available by transmission, or through the intervention of a mass of elastic fluid. This is the case in the Cornish engine, and, in further illustration of the subject, it may be important to consider some cases of the impact of one elastic fluid on another. With this view I shall select some experiments made by Mr. William Greener,

gun-maker, of Newcastle-upon-Tyne. His object was to determine the distance to which a ball would be projected from a barrel, open at both ends; or, what is the same thing, to ascertain the repulsive power of the atmosphere against the explosive force of gunpowder. For this purpose, he prepared two barrels of 6 feet, and 8 feet, in length, their bore being No. 15 of the trade, or somewhat more than $\frac{5}{8}$ths of an inch diameter. From the various, and curious results obtained by Mr. Greener, I will select the following from the 8 feet barrel, as most suitable for my purpose.

A cartridge, containing three drams of powder, was pushed down to a touch-hole pierced at 32 inches from one end, so that a column of air remained behind the cartridge of 64 inches in length, acting as a breech. A ball, which fitted the bore well, but not over tight, was pushed down upon the cartridge. The barrel was then placed at a slight elevation, and fired. The ball was projected considerably above 200 yards. Mr. Greener observes, "This effect was not quite equal to that commonly obtained from a breeched barrel, as I used a smaller charge of powder; but it nearly equalled the resisting strength of the barrel, as was proved by another experiment."

In this last he employed four drams of powder, and placed three balls above the cartridge. On firing, the barrel burst, immediately behind the touch-hole or point of inflammation, on the column of air side. On this phenomenon Mr. Greener remarks, "It shows the repulsion of the particles of air driven back upon themselves to be equal to the strength of the barrel; and, no doubt, if the barrel had been strong enough, and the charge of powder increased, the velocity of the bullet would have been as great, when propelled by the reaction of the explosive force against this column of air, as if the barrel had been closed with a solid breech."—"It must be observed," he adds, "that it is only at a high velocity this wonderful arrangement of nature to curb our longing for extremes is shown. The mean velocity of the ball must at least equal that at which sound travels, for a perfect exhibition of the propulsive power of gunpowder acting against an air breech."

These experiments and remarks of Mr. Greener's are replete with instruction: they show that an aëriform elastic fluid, in a state of repose, closely resembles a liquid. When slowly acted upon, the mobility of our atmosphere is such as to yield easily in all directions, and to offer but little sensible resistance to the movement of objects through it; but, when only partially confined, and acted upon violently, and suddenly, the difficulty of displacing it is

great. They show, also, that upon the velocity of the stroke depends the effect produced by the impact of aëriform fluids; and that a close analogy exists between the law of percussive action in aëriform fluids and solids, though the former must be considered as imponderable, and perfectly elastic.

Mr. Greener informed me that the thickness of the barrel, where the explosive force acted, was $\frac{5}{32}$ds of an inch; and he estimates the instantaneous pressure exerted as equal, in this case, to about 1,200 lbs. per square inch. In all these experiments but little smoke, and very few sparks, issued from the open breech; showing, together with the effect upon the projectile, that, for an instant, the air must have been most powerfully compressed: for, to use Mr. Greener's pertinent and expressive language, "the repulsion of the particles of air, driven back upon themselves," opposed a resistance nearly equal to that of a solid breech.

Scarcely a sound succeeds the firing of powder in these open-ended barrels; there is little or no vacuum to be filled up, as the air follows the gunpowder gas as fast as it escapes; and "it is thus proved," Mr. Greener observes, "that the report of a gun is altogether the work of air."—[Note K.]

Difficulty of Measuring the Percussive or Instantaneous Force of Aëriform Fluids.

Table 2 contains certain results, selected from the experiments with the indicator and mercurial column, affording the nearest *approximate* measures obtained of the pressure due to the steam's momentum at different elasticities. But Mr. Greener's extreme experiment with another elastic fluid, gunpowder gas, warns us both of the difficulty of contriving and of placing an instrument so as to indicate truly the force of percussion, or of the instantaneous action derived from aëriform fluids; and it induces doubts of the sufficiency of the instruments for that purpose employed in the preceding investigation.

In the experiment at Mr. Perkins's, atmospheric air formed the medium by which the force of the steam's instantaneous action was transmitted to the piston of the indicator, and exhibited by the index. It seems, however, certain that if a similar instrument had been applied to the extremity of Mr. Greener's gun-barrel, it would not have truly marked the force which operated on the other end of the column of air next the cartridge, at the instant of the explosion of the gunpowder. The phenomena of the projection of the ball, and of the bursting of the barrel, disclose the remarkable fact that the cushion of air, against which the force acted, was not displaced; or not to such an extent as to drive it out of the barrel. It is, therefore, evident that, at some certain distance

from the point of explosion, in a barrel of greater length (and possibly in the one in question), no compression of the air would take place; and that an indicator placed at that spot would be unmoved by the explosion. Mr. Greener ascertained for me that the flame of the explosion, in the eight feet barrel, had only extended six inches from the touch-hole into the column of air. It may then be concluded that the atoms of air which lay nearest to and first received the stroke of the generated gas, became so condensed as to offer a resistance like a solid or liquid body, and that a very small portion of the initial momentary pressure was transmitted to the air at the extremity of the barrel. The exertion of a small amount of force, slowly applied, would expel all the air; but if the velocity of the impinging force be greater than that at which an aërial wave can be propagated along the barrel, then the air will present a base against which reaction can take place: and hence it is that the air served as a breech for the projection of the bullet.

Of the velocity of steam we know little, at any elasticity, but, it must be immensely great when expanding from a pressure of 26 atmospheres to that of 1 atmosphere; and it is possible that the column of air (about three feet in length) enclosed between the cock of the generator and the piston of the indicator, in the experiment at Mr. Perkins's, may not have transmitted the whole force of the steam's instantaneous action to the piston: it is possible that, in this case, as in Mr. Greener's gun, a portion of the force may have reacted against the confined air, and not have reached the piston. The same remarks apply to the mercurial gauge, from which the air in the pipe and above the mercury could not be expelled; and it is probable that if the glass tube had been of larger dimensions, particularly at the end immersed in the mercury, and if the steam pipe had been shorter, and larger at its junction with the cistern, the effect would have been increased.

I am the more earnest in drawing attention to the inadequacy of the instruments for determining the abstract amount of the action in question, as the experiments on the cylinder-cover convey to my mind an impression that the force which sprung it (and which has broken so many very massive covers) must have considerably exceeded in amount the pressure which would appear to have acted, as deduced from the ratio of the results in Table 2. A pressure of 54·7 lbs. per square inch, the steam being at rest, produced no change in the parallelism of the cover; yet, the momentum arising from steam of 5 lbs. less elasticity, viz., of 49·7 lbs. per square inch, sprung the cover $\frac{5}{32}$ds of an inch at

the centre. By applying the ratio of 1·386 to 1, to 49·7 lbs., a force is obtained of 68·7 lbs. per square inch as the amount of the pressure due to the steam's momentum. I am disposed to doubt the adequacy of this force to spring the cover to the extent measured. Seeing that a quiescent force of 54·7 lbs. per square inch did not change the figure, it seems to me very questionable whether a force greater only by 14·0 lbs. per square inch would suffice; and whether the additional force of 16·6 lbs. per square inch would have sprung the cover to the further extent of $\frac{3}{32}$ds of an inch. However this may be, doubt cannot extend to the fact or degree of the springing; nor, I think, to the soundness of the conclusion that an amount of force, equal to that which acted upon the cylinder cover, must also have operated upon the piston of the engine, and have produced a corresponding dynamic effect.

The steam engine indicator, as commonly applied and as used in the foregoing experiments, is open to many objections as an instrument for detecting the value of forces acting with great suddenness, and velocity. The influx of the steam is usually much throttled by the contracted dimensions of the cock, and a column of air exists above, and in some cases below, the piston, which must impair its accuracy for this purpose. No reliance could be placed on its indications of such a force as that which sprung the cover of the Fowey Consols engine, unless it were so constructed and applied as to have the bottom of its piston coincident with the interior face of the cover on the instant of the steam's admission. Other precautions will also be suggested by these remarks as regards the construction and use both of this instrument, and of the mercurial gauge, in conducting experiments for measuring the percussive force of aëriform fluids.—[Note L.]

Phenomena of Steam-boiler Explosions.

In conclusion, I would suggest whether some of the hitherto unexplained phenomena, attending the explosion of steam-boilers, may not be explained by the foregoing experiments on the effect of instantaneous action.

The greater part of these phenomena are more or less characterized by suddenness; few exhibit signs of the operation of a gradually increasing or of a continued force. We have examples out of number of boilers, weighing many tons, embedded in masonry, and fastened down by various attachments, being projected to a vast height or distance without the slightest warning. I am unable to comprehend that the simple elastic force of steam can be made to separate a boiler into two parts, projecting the one into the atmosphere and

leaving the other quiescent. I have still greater difficulty in understanding how the character of a projectile can be given to such a body as a boiler, without its having received, originally, a sudden impulse, using that term as implying momentary action, in contradistinction to continuous pressure. It is conceivable that a gradual increase in the elasticity of confined steam might dislocate some plates of a boiler, and even open several seams, at one and the same time; but in most of these cases the substance of plates is divided—in many cleanly and truly, as if done by a hammer and chisel, or shears. Such is not the effect of an uniformly increasing tensile strain: under these circumstances, extension of the metal would precede rupture. The development of some extraordinary and sudden power can alone, in my opinion, account for such effects.

As regards the projection of the whole body of a boiler from its seat, the operation of an immense force, combined with instantaneousness of action, seems necessary for the production of the effect.

Instances have occurred in which an internal fire-tube has been projected in one direction, and the cylindric shell in the opposite one. These and many other remarkable facts respecting the rupture of boilers, on the instant of starting and stopping engines, as also on the instant of suddenly closing and opening safety valves, may, it appears to me, be readily explained by some of the views here advanced.

The causes of these explosions are numerous, often insidious; and it is a subject involving so many considerations relative to the material and structure of boilers, their management, the action of the fire, and the action of sudden disturbances both within and without the vessel, that I can at present only allude to them as intimately connected with the subject of this communication.

JOSIAH PARKES.

12, Great College Street, Westminster,
April, 1841.

NOTES AND ILLUSTRATIONS.

(A.)

"In framing a theory which shall render a rational account of any natural phenomenon, we have *first* to consider the agents on which it depends, or the causes to which we regard it as ultimately referrible. These agents are not to be arbitrarily assumed; they must be such as we have good inductive grounds to believe do exist in nature, and do perform a part in phenomena analogous to those we would render an account of; or such, whose presence in the actual case can be demonstrated by unequivocal signs. They must be *veræ causæ*, in short, which we can not only show to exist, and to act, but the laws of whose action we can derive independently, by direct induction, from experiments purposely instituted; or, at least, make such suppositions respecting them as shall not be contrary to our experience, and which will remain to be verified by the coincidence of the conclusions we shall deduce from them, with facts. For example, in the theory of gravitation we suppose an agent, viz., force, or mechanical power, to act on *any* material body which is placed in the presence of *any* other, and to urge the two mutually towards each other. This is a *vera causa;* for heavy bodies (that is, all bodies, but some more, some less) tend to, or endeavour to reach, the earth, and require the exertion of force to counteract this endeavour, or to keep them up. Now, that which opposes and neutralizes force *is* force. And again, a plumb-line, which, when allowed to hang freely, always hangs perpendicularly, is found to hang observably aside from the perpendicular when in the neighbourhood of a considerable mountain. Moreover, since it is a fact that the moon does circulate about the earth, it must be drawn towards the earth by a force; for if there were no force acting upon it, it would go on in a straight line without turning aside to circulate in an orbit, and would, therefore, soon go away and be lost in space. This force, then, which we call the *force* of gravity, is a *real cause*."—*Discourse on the Study of Natural Philosophy*, p. 197. *Sir J. F. W. Herschel.*

(B.)

The two following indicator diagrams will sufficiently illustrate the steam's initial action :—

Fig 1 is a copy of No. 39, table 1, taken from the smaller of the two cylinders of Mr. Humphrys's engine. A simultaneous observation was made on the pressure existing in the boiler, on the instant of taking the diagram, by means of a thermometer graduated for temperatures and pressures.

Fig. 2 is a copy of No. 7, when the valve was *handed*, (referred to, page 415,) the steam in the boiler being, at the instant, exactly of atmospheric pressure. To ensure accuracy as to the elasticity in this boiler, the steam was blown off till it issued only at the boiling point.

Fig. 1.

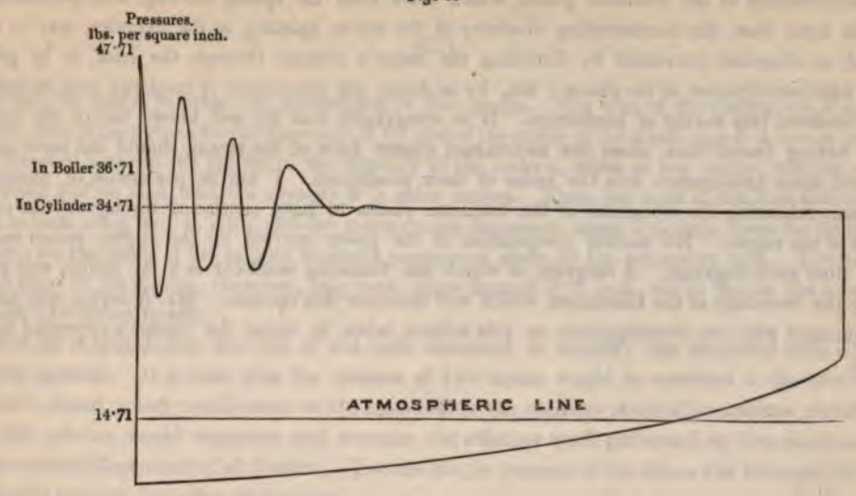
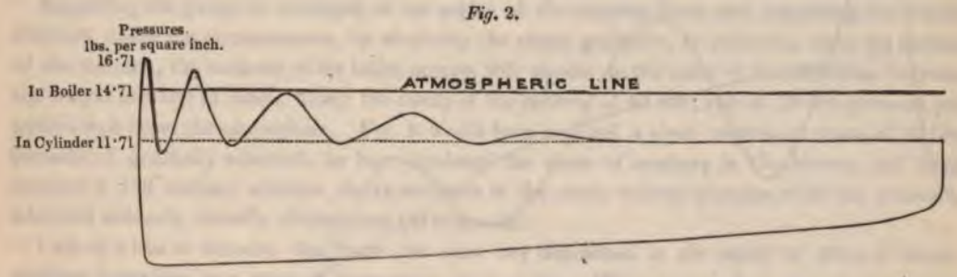


Fig. 2.

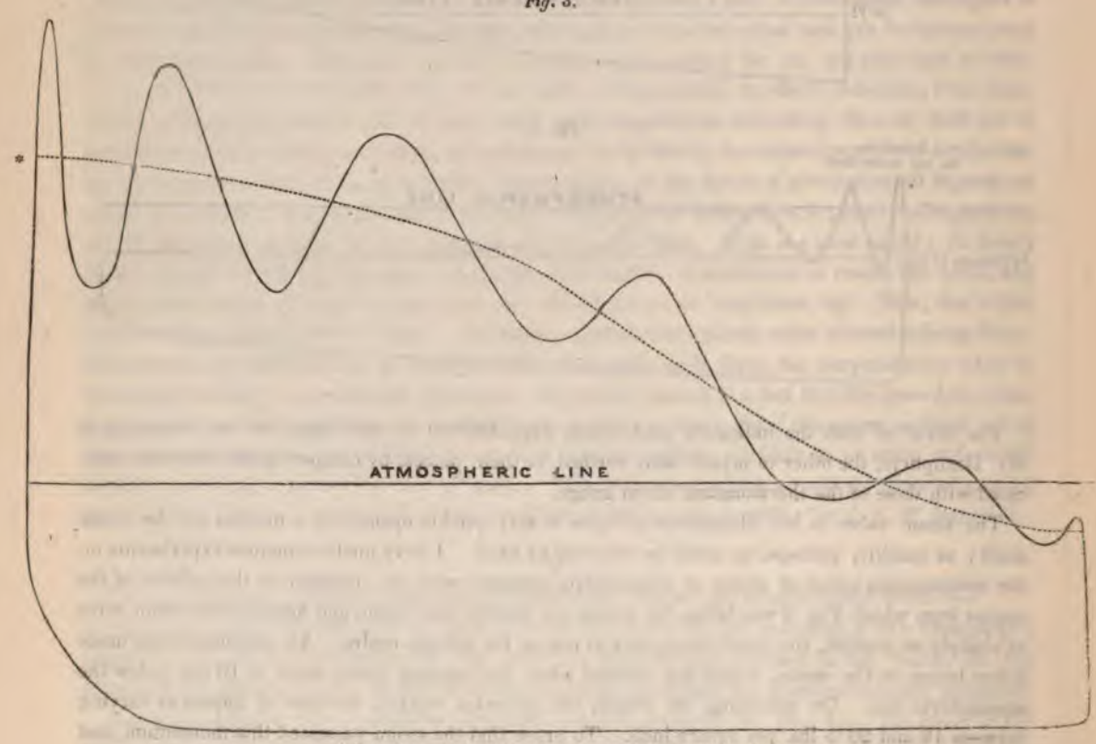


The scale of both the indicators used, which were different in each case, the one belonging to Mr. Humphrys, the other to myself, were verified in their places, by comparing the pressures indicated with those of the thermometric steam gauge.

The steam valve in Mr. Humphrys's engine is very quickly opened by a tumbler on the crank shaft; as quickly, perhaps, as could be effected by hand. I have made numerous experiments on the instantaneous action of steam at atmospheric pressure with the indicator on the cylinder of the engine from which Fig. 2 was taken, by setting the throttle valve open, and *handing* the steam valve as sharply as possible, the crank being kept at rest on the bottom centre. An exhaustion was made before laying on the steam, which was effected when the indicator piston stood at 10 lbs. below the atmospheric line. On admitting the steam, the indicator marked the force of impact as varying between 19 and 20·5 lbs. per square inch. To prove that the steam possessed this momentum, and that the piston of the instrument did not ascend by virtue, solely, or principally, of any acquired momentum of its own, higher than was due to the steam's action upon it, a small clamp was fixed on the piston rod, which rested on the cover, and kept the piston suspended to a height corresponding with the pressure in the boiler. The result was much the same; the piston was, in every case, lifted, and it instantly descended on the clamp.

The vibrations of the indicator piston, which arise from the spring exerting, after percussion, a greater force than the counteracting elasticity of the steam existing in the cylinder, may be controlled, or altogether prevented by throttling the steam's passage through the cock, or by giving some additional friction to the piston; but, by so doing, the instrument is impaired, and its indications rendered less worthy of confidence. It is remarkable that the well known fact of the instrument having traced lines, above the ascertained elastic force of the steam, should not have earlier induced some examination into the cause of their production. I am in possession of numerous diagrams exhibiting the vibrations of the indicator piston to have continued throughout the entire stroke of the engine. No faithful computation of the power exerted on the engine piston can be made from such diagrams. A diagram, of which the following wood-cut is a fac simile, was given me by the Secretary of the Institution, which will illustrate this opinion. Mr. Atherton was totally unacquainted with my investigations on this subject, when he wrote the remarks appended to the figure.

Fig. 3.



* Imaginary line of the steam's elasticity in the cylinder.

"Diagram showing the rapidity and force of the stroke of high pressure steam exemplified by its driving the piston of an indicator far above the point corresponding to the pressure of the steam even in the boiler, and the consequent oscillation with which the piston of the indicator moved."

(Signed) CHARLES ATHERTON.

(C.)

It may be useful to state the dimensions of this gauge. The area of the cistern was 6 square inches; that of the perpendicular glass tube 0·03, and of the orifice of the steam pipe 0·4 square inches. The distance between the surface of the mercury in the cistern, when at the fullest, and the cover, was about $\frac{3}{4}$ of an inch, and the capacity 4·5 cubic inches. Thus, the areas of the tube and cistern were to each other as 1 to 200; and the mercury was depressed about 0·6 inch, when the difference between the two levels of the column showed a pressure of about 59 lbs. per square inch. The motion of the mass in the cistern, therefore, only took place through 0·6 inch, whilst that in the tube was through 119 inches nearly.

Let it be supposed that the end of the tube immersed in mercury was furnished with a valve opening upwards. It is clear that the column of 119 inches would be sustained in the tube when the valve closed, which would occur on the instant after the mercury attained its extreme elevation; and this column would represent and measure the effective work performed by that single stroke of the steam, independent of all friction. Yet, the simple pressure of the steam was balanced by, and could only support, 80 inches of mercury.

Regarding the gauge, so arranged, as an engine of the simplest form, and comparing the results obtained, under the circumstances, by admitting the steam gradually, or suddenly, upon the surface of the mercury, the economy of the latter process will appear in the value of the difference between the weight of water as steam, filling the cavity of the cistern, at 40 lbs., and at 59 lbs. pressure per square inch above the atmosphere. For, it would have required a given volumn of steam of 59 lbs. pressure, if gradually admitted, to have displaced the mass of mercury in the cistern, and have elevated it 119 inches; whereas, the expenditure of the same volume of steam of 40 lbs. pressure, admitted suddenly, actually effected that performance.

I am at a loss to conceive that there can exist any distinction in the action or effect of steam, whether operating on a mass of mercury or on a piston. We may not, from circumstances into which it is unnecessary to enter, realize in the steam engine the whole of the force, however applied, but a like method of applying the steam must, as it seems to me, produce like results in both cases.

(D.)

A phenomenon which occurred during some experiments on the steam gun at Mr. Perkins's seems to me to be strictly analogous to the phenomena exhibited by the indicators, and the mercurial gauge.

I observed that, occasionally, a bullet stuck in the barrel; that it was not discharged, though the communication with the generator remained fully open. On shutting, and then re-opening the steam valve, the ball passed; in one case, three discharges of steam were necessary for its projection. Now, in that short interval of time, not a second, between the operator observing the non-expulsion of the bullet, and his closing and opening the valve, I cannot imagine any increase in the steam's elasticity

to have taken place in the generator, sufficient to overcome the existing resistance. The ball fitted the barrel, as a piston in an engine cylinder; steam of 26 atmospheres' pressure was unable to move it; not a particle of steam issued from the muzzle; yet, by merely shutting off, and sharply re-admitting the steam, the ball was expelled. This occurrence took place several times. Whether these balls were not, originally, truly spherical; or had been flattened by the steam's first impact upon them; or had some roughness on their surface; or, whatever may have been the immediate cause of their sticking in the barrel, certain it is that steam of an elasticity identically the same with that which had proved insufficient, did, on its *sudden* re-admission, expel the balls with violence.

For a perfect comprehension of these facts by persons who are not familiar with the apparatus, it may be well to explain that the ball is dropped into the barrel through a perpendicular tube at the breech, and rests in a chamber close to the steam valve, which may be called the breech. The bore of the barrel is parallel throughout, and $\frac{5}{16}$ths of an inch diameter. The diameter of the valve is $\frac{5}{8}$ths of an inch; its area, therefore, is four times that of the barrel, so that there is no throttling, or *wire drawing* of the steam on its admission.

A ball being introduced into the barrel, the valve is rapidly opened by a handle. In the cases under examination, the first action of the steam is to drive the ball some space along the barrel. It there remains unmoved, and hermetically fits the tube. The steam, at this instant, is in a state of rest, still acting against the ball, but exerting only the simple elastic force of the steam in the generator; it possesses no momentum. The operator, perceiving that the ball has not been expelled, shuts off the steam, and again suddenly admits it. The expulsion of the ball is now effected, having been struck by a force superior to the steam's simple pressure, viz. by its momentum. The first blow sufficed to drive the ball a certain space; but, when the momentum, communicated to it by the steam's percussive or instantaneous action, was expended, the force remaining in action fell below the resistance, and the motion of the ball ceased; a second blow was requisite for its complete expulsion. Such appears to me to be the explanation of this phenomenon.

(E.)

No apparatus for opening steam valves with rapidity, and, therefore, for letting loose the steam upon the piston of an engine with the full velocity due to the difference of the elasticities existing in the boiler, and in the cylinder, has yet been applied so effective as the cataract. And, perhaps, no description of valve, hitherto constructed, opposes so little resistance as the Cornish double-beat skeleton valve; nor one which discharges so much steam for an equal amount of lift.

The opening of this valve, by the falling of a weight, is as nearly instantaneous as the discharge of fire-arms by the action of a hair trigger.

To the introduction of this valve and its enlarged dimensions, combined with the use of the cataract, much of the increased performance of modern, over older, Cornish engines, is attributable. An authentic history of the invention of this valve, and of its original construction, with the changes it has undergone in form and dimensions, relatively to the pressure of steam in the boiler, and to the area of the cylinder, would not only be highly interesting, but illustrative of one principal cause of the progressive advance in the economy of the pumping engine.

(F.)

Though having only referred to *single* acting, the observations apply equally to other engines of the same kind which are *double* acting, but not rotative. Blast engines are frequently so constructed, and also some pumping engines.

It is not absolutely necessary for the good working of a crank engine that the steam should be admitted previously to, or on the instant of the piston coming to rest, at each end of the stroke, though such be the practice. The steam might be let on after the crank has passed the centre, but, for obvious reasons, its instantaneous or percussive action would not, even then, be so efficiently exerted upon the piston, as under the more favourable circumstances of the single-acting engine.

(G.)

The "breathing," or springing upwards, of cylinder covers on the steam's admission is familiar to engineers. It is, I believe, the present practice in Cornwall not to strengthen them with ribs, but to give a greater thickness of material, by which a sufficient elasticity is secured, and fracture avoided.

This experiment on the effect produced on the cylinder cover by steam of nearly similar elasticity, when in motion, and when at rest, is, in the language of Bacon, a "*glaring instance*" of the "*crucial*" kind; it is a definitive experiment of the most satisfactory description. We obtain from it no *measure* of the difference between the forces under investigation: but it leaves no doubt on the mind as to the one being superior to the other; nor, as to the *character* of the greater action. Sir John Herschel, in illustrating Bacon's philosophy, says, "One of what he calls '*glaring instances*' has just been mentioned. In these the nature, or cause inquired into, (which in this case is the cause of the assumption of a peculiar external form, or the internal structure of a chrystal,) stands naked and alone, and in this eminent manner, or in the highest degree of its power. No doubt such instances as these are highly instructive; but the difficulty in physics is to find such, not to perceive their force when found." And again, "the surest and best characteristic of a well-founded and extensive induction is, when verifications of it spring up, as it were, spontaneously, into notice, from quarters where they might be least expected. Evidence of this kind is irresistible, and compels assent with a weight which scarcely any other possesses."—*Discourse on the Study of Natural Philosophy*, pp. 170, 184.

(H.)

In consequence of the greater intensity of the steam's impact, when the piston starts from nearer the cover, the engine requires regulation, and the throttle valve is more closed, since a less pressure of steam is requisite to complete the stroke. (See also ante, p. 272.)

A small increase in the length of stroke might be occasioned by starting the piston from nearer the cover, but the volume of steam in the cylinder when its influx is stopped would be unaltered,

as the period of closing the steam valve remains unchanged. The fact of several millions more duty being performed under these than under the ordinary circumstances, assures us that the steam, which fills the cylinder at the termination of the stroke, is of less elasticity than when the piston is started at a greater distance from the cover, since the higher performance, or resulting economy, consists only in having used less water as steam to effect the stroke. Steam, therefore, of less density filled the space of the cylinder when its influx was stopped; yet, the same resistance has been overcome, as by an equal volume of steam, of higher elasticity, under the ordinary circumstances.

(I.)

The second instance occurred at Mr. Stuteley's saw mills, Bankside. On visiting the place, I found the statement, as originally made to me by Mr. Edward Humphrys, to be confirmed by the engineer, who had provided against the loss of the mercury, in future, by interposing a second, and longer syphon-pipe, between the gauge and condenser, to retain the mercury if again blown over.

By referring to Note C, and the experiment to which it relates, it will be apparent that, had not the cistern contained mercury enough to balance a force of 59 lbs. per square inch, the whole of it would have been evacuated, by steam of an elasticity of 40 lbs. suddenly admitted.

I lately had an opportunity of measuring the amount of oscillation produced on a mercurial syphon gauge attached to a Cornish engine. The gauge was fixed on the steam pipe in the engine-house, between the throttle valve and boilers. The steam in the boilers was at 40 lbs. above atmospheric pressure. On each admission of steam, and during its flow into the cylinder, the gauge exhibited the pressure in the pipe to be 37 lbs.; on the instant of closing the steam valve, the mercury rose to 44 lbs., settling again to 40 lbs. before the succeeding stroke.

Mr. Lowe stated, (see "Minutes of Proceedings," 1841, p. 150) in the conversation which followed the reading of this paper at the Institution, that, on opening the stop-cock connecting a main gas pipe with his water-pressure gauge, the water experienced an instantaneous action, equal to a column of six inches; though the simple pressure of the gas in the pipe only balanced four inches. This, and the depression observed in his gauges when the gas is in motion along the pipes, are instances showing the identity which obtains in the action of different aëriform fluids.

(K.)

These, and other, experiments, equally remarkable for their novelty and ingenuity, are contained in a work recently published by Mr. Greener, entitled the "*Science of Gunnery*," which records a mass of information interesting to the philosopher and engineer, as well as to the gunner and sportsman. The quotations above given are from Mr. Greener's correspondence with me, previous to the publication of his work, and not exactly in the words of his text, pages 37, 38, 39, by which it appears that his researches on the subject have been considerably extended.

The action of gunpowder gas, on the instant of its generation, seems to possess characteristics very analogous to that of steam suddenly put in motion. The following experiment is related by Mr. Robins, who first discovered the permanently elastic nature of gunpowder gas:—

"When gunpowder is fired in an exhausted receiver, the mercurial gauge instantly descends upon the explosion, and as suddenly ascends again. After a few vibrations, none of which, except the first, are of any great extent, it fixes at a point which indicates the density of the enclosed gas."—*New Principles of Gunnery.* The phenomena of the action of steam upon mercurial columns, and upon the indicator, are exactly described by these words.

It has, long since, been demonstrated that the report of a gun is produced by the rush of air into the barrel to replenish the vacuum. The residual gas left in a barrel, after the explosion, is estimated at about 1-8th of the pressure of the atmosphere, the velocity and momentum of which, on re-entering, must, therefore, be very considerable. These, and many other similar phenomena, which offer themselves to the recollection, furnish the fact of an audible blow being given by the impact of an elastic fluid on a solid, and also by the concussion of opposing elastic fluids.

Several instances have been brought to my notice of an audible blow, or shock, similar to that of the hydraulic ram, taking place when a cock is very rapidly shut in a pipe through which steam is flowing into the atmosphere. I witnessed one instance of this nature at Mr. Thomas Cubitt's works, Thames Bank. It occurs on quickly shutting a cock in a wrought iron pipe of considerable length, and about an inch bore, conveying steam through the building. Whether the noise and shock were occasioned by the sudden stoppage of the current of steam on one side, or by the sudden entrance of atmospheric air on the other, caused by the vacuum, partial or perfect, which might result, certain it is that the closing of the cock was instantly succeeded by an audible sound (as if produced by a blow) and by a tremor of the pipe, which resulted from the momentum of one or other of these elastic fluids.

(L.)

Whilst making the experiments at Mr. Perkins's, I happened to grasp the pipe close below the indicator, in order to steady my body, and bring my eye as near the dial-plate as I could, to observe the motion of the index, on suddenly letting on the steam of 26 atmospheres. It immediately afterwards occurred to me that my hand would have been burnt if the steam had mixed with the air, and reached the spot where my hand was placed; but this was not the case, as the pipe, though hot from conduction, could be grasped (at the commencement of the experiments) notwithstanding that the steam pipe, on the other side of the cock, had the temperature due to that of the steam. The pipe full of air was, as above stated, about a yard in length; and this phenomenon clearly shows that the force of the steam's instantaneous action, or of such portion of it as reached the indicator piston, was transmitted by the air in mass, as if a diaphragm, or piston, had separated the columns of steam and air. There was no time for the diffusion of the gases and mixture of their particles. The space and substance of the cock-plug alone divided the two columns; and the steam, on the sudden and full removal of this obstacle, must have effected its impact on the air, much after the manner of a solid or liquid body moving at the same velocity. It seems to be a settled physical fact, that a pulse of air cannot be made to move at a greater velocity than that of sound, or at a rate exceeding about 1,300 feet per second. It is distinctly seen from the experiments detailed.

that the velocity and momentum of steam increase with its elasticity, but the phenomena exhibited by the impact of one elastic fluid on another, coupled with our ignorance of the velocity of steam, and with the fact of its force being transmitted to the meters, in every case, by an elastic fluid, show that the construction of an instrument, as well as its position, for truly measuring the amount of persussive action, require much consideration.

J. P.

ERRATUM.—I take this occasion of correcting an error of computation in the last paragraph of page 153 of this volume. It is there stated that the ineffective portion of the steam consumed by the engine in question was only about $\frac{1}{40}$th, or $2\frac{1}{2}$ per cent. of the total quantity. The loss, however, amounted to about 20 per cent.; the ratio of the volumes of steam and water consumed for the observed pressure being 911, instead of 771, as in the text. This error in no degree affects the quantities set down in Table VI. respecting the performance and consumption of the engine; it has reference only to the observations contained in the paragraph.

J. P.

XIX.—*On the Circumstances under which Explosions frequently occur in Steam Boilers, and the Causes to which such Explosions may be assigned.*

By CHARLES SCHAFHAEUTL, M.D., Assoc. Inst. C. E.

Read March 30, 1840.

Of the explosions which occur in steam boilers, it may with reason be assumed, that a very large proportion must be assigned to some other cause than the simple pressure of the steam due to the accumulation of a surplus quantity in the boiler, or the overloading of the safety valve, since many cases of explosions have occurred in which it has been ascertained beyond all doubt that the safety valve was in perfect order, and also cases in which it had acted immediately before the explosion, or was even in action at the instant of that occurrence.

Undoubted and indisputable facts of this nature, and the considerations hereafter referred to, show that these calamitous occurrences must be referred to other causes than the gradual increase of pressure in the steam, or defects in the ordinary safety valve.

Bursting due to simple pressure.

The pressure which hollow vessels will sustain without bursting, so long as that pressure is added gradually and by small accessions, would scarcely be suspected or credited without trial. In an extensive series of experiments, I found that the substance of metallic boilers was almost uniformly reduced in thickness by the action of a continuously increasing pressure within. Boilers under these circumstances exhibited no signs of rending; and in several cases the boiler sustained an internal pressure double that which it might reasonably have been expected to bear.

In an experiment of Cagniard de la Tour, a glass tube one-fourth part filled with water was hermetically closed, and inserted in a bath of metallic zinc, which was carefully heated until its temperature was sufficient to convert the water entirely into steam. The temperature of this bath, as ascertained by Daniell's pyrometer (a proper correction being made for the expansion according to the coefficient of Dulong and Petit) was 755° F. According to the formula of the same eminent philosophers, the pressure of the steam at that temperature would have been 4382 lbs. per square inch, but with a relative

volume of 10·24; therefore only 0·39 of the water would have been converted into steam; but as, according to Cagniard de la Tour, the whole of the water was converted into steam, a temperature of at least 945° F. would have been necessary to reduce the relative volume of steam to 4, and a corresponding pressure of 13,000 lbs. per square inch, or more than 866 atmospheres pressure would have existed in the tube.

In repeating the experiments of Cagniard de la Tour, I subjected glass tubes of from 1 to 2 inches in length, containing steam of an elastic force of not less than 400 atmospheres to an external pressure in the bath of zinc, of from 1 to 40 lbs., without breaking them; and the tubes under this internal pressure would bear considerable pressure with an iron rod so long as the pressure was gently applied; but if one end of the rod were pressed against the glass tube, and the rod made to vibrate longitudinally by rubbing it with a leather glove covered with resin, the tube was invariably shattered to pieces.

In this case we have a distinct illustration of the effect of something besides the simple excess of pressure of steam in a boiler, in causing a disruption of its parts, or an explosion. In a great variety of experiments which I made, with the view of elucidating these points, I found that any vibratory motion, or any motion communicated to the boiler at intervals in an irregular manner, were extremely likely to produce a disruption of the parts, or an explosion. These considerations, together with the circumstance that in very many cases of explosions we may fairly presume the safety valve to have been of no use, may lead us to consider whether in all cases of explosions there may not have been in action a force momentary in its nature, tearing asunder the plates of the boiler at the instant of its generation, and before there was time for its transmission to the safety valve.

Suggested causes.

The existence of a force of this nature, sudden in its origin, and instantaneous in its duration, has suggested itself to many minds, and some persons have been inclined to ascribe it to the presence of hydrogen generated by the decomposition of water; but besides the great difficulty of generating a large quantity of hydrogen in steam-boilers, it could never cause an explosion without the presence of oxygen or atmospheric air, and even this explosive compound would not take fire when mixed with $\frac{7}{10}$ths of its own volume of steam, as I have proved conclusively by the following experiment:—

Experiment.

I introduced into the shorter end of a glass tube bent in the form of a syphon a mixture of one volume of hydrogen and two volumes

of atmospheric air. The gases were then confined in the tube by means of mercury, and subjected to a pressure of an atmosphere and a half. A small quantity of water, the weight of which was accurately calculated according to the required volume of the gas in the experiment, was allowed to be absorbed by a texture of asbestos, and then by means of a wire the asbestos was conducted through the mercury into the mixture of gases contained in the tube. The tube was then placed in an iron cylinder filled with mercury, and kept at a temperature of about 236° F.; the closed end of the glass tube was then carefully heated by means of a blow-pipe to a red heat, for the purpose of causing an explosion. The following were the results of the experiments:—One volume of the explosive mixture with $\frac{1}{10}$th of its volume of steam exploded and broke the tube. The same result took place with $\frac{2}{10}$ths of its volume of steam, and with almost the same violence; with $\frac{3}{10}$ths of its volume of steam, but with less violence; with $\frac{4}{10}$ths of its volume of steam, but with still less violence; with $\frac{5}{10}$ths, or one half of its volume, a very feeble explosion took place just as the heated glass tube began to be extended by the pressure of the gas. With $\frac{6}{10}$ths of its volume of steam only one explosion took place, though the experiment was tried six times; and with $\frac{7}{10}$ths, and all greater proportions of steam to gas, up to two volumes, no explosion could be procured.

Conclusions from preceding.

From these experiments we may conclude that, even if an explosive mixture were formed in a boiler, no explosion could ever take place unless its volume were at least double that of the steam, which under any circumstances is highly improbable, and unless the boilers were almost dry, next to impossible.

Mr. Parkes's views.

The very ingenious paper by Mr. Josiah Parkes on the action of steam in Cornish engines induced me to turn my attention to the effect of the sudden development of an action in steam-boilers, and to investigate analytically the laws of the concussion of liquid and gaseous bodies; the details of these investigations I shall hope soon to be able to lay before the Institution. On the present occasion I have to offer some practical observations on the effect of sudden changes of pressure within a boiler, due to the sudden generation of steam as distinguished from the uniform and regular increase of pressure due to accessions of steam gradually generated and accumulating by degrees.

There is at present only one mode in use for converting water into steam, viz. by successively adding a small proportion of caloric to a relatively immense

body of liquid; but all the heat imparted in successive intervals of time to a given quantity of water might be imparted in one unit of time. For example, if a drop of water be put on a blacksmith's anvil, and a bar of iron be heated to a white heat, or until it becomes covered with a layer of liquid slag, and the bar be laid in this state on the drop of water and struck with a heavy hammer, the liquid slag on the surface of the iron will communicate its caloric at once to the water, the slag becoming solid at the same instant that the water is converted into vapour, with a loud report similar to that of a gun. This is an extreme case; but a nearly similar occurrence may take place in a steam-boiler where a quantity of water is suddenly thrown in contact with the overheated bottom plate of the boiler, either from a sudden motion of the vessel, or from a portion of incrustation giving way, and opening at one instant a passage for the water to the red hot plate.

In a high pressure boiler, the sudden separation of a deposit causes first a diminution of the quantity of evaporation, and immediately afterwards a sudden bursting up of steam, which very often endangers the boiler. This separation of deposit may be occasioned or promoted by a sudden opening of the safety valve, or by a rapid increase of heat causing a sudden ebullition in the water.

Consequences of sudden increase of pressure.

In all cases where a sudden explosive development of steam takes place in the boiler, the first and principal part of its action is directed against the place of its development; that is, against the bottom, or even the sides of the boiler, then spreading itself through the water, and finally through the steam to the top of the boiler.

Let us consider this last circumstance more closely. The boiler is supposed to be filled with two fluids, water and steam, superposed in equal or unequal heights or depths: if we now assume that a small body is made to explode on the surface of the water, the moving particles of the generated gas will communicate their motion of course at the same time to the molecules of water as well as steam nearest to them; but as both fluids are elastic and compressible, though in very different degrees, the imparted motion will not be transmitted instantaneously through the whole mass: the molecules of the water and steam nearest to the exploding body will be compressed only for a small depth, which compression will communicate itself successively to the next and succeeding molecules of the fluids, till the motion like the undulation of a wave finally arrives at and is communicated to the sides of the boiler. But the velocity

with which this takes place depends on the power the fluids in question have for conducting undulatory motions, that is, on their compressibility. According to Laplace's formula, we obtain for the conducting power of steam at a pressure of four atmospheres and 294·1° F., 1041·34 feet per second, and for water 6036·88 feet. The ratio of the different conducting powers, or of the velocities of the waves in the respective media, is as 1 to 5·7, that is, the wave created by the explosion or the sudden pressure is transmitted through the water 5·7 times faster than through the steam, and consequently the action will be exerted on the bottom and sides of the boiler in $\frac{1}{5 \cdot 7}$ of the time that it is exerted on the equidistant parts in contact with the steam. Now as the safety valve can only receive the action transmitted through the steam, the part of the containing surface at which it is situate will not in general be the first part of the surface to receive the action so transmitted.

From this it follows that the first effect, spreading from the centre of the boiler or the surface of the water, will in general reach the sides and bottom of the boiler sooner than the safety valve; and if we consider it to originate at successive depths of the water, the denominator of the fraction expressing the ratio of transmission will decrease as the numerator increases. When the explosion takes place near the bottom, the time in which the wave or sudden pressure reaches the safety valve is now the sum instead of the difference of the two velocities.

It may be objected that, notwithstanding the relative difference between the two velocities, the quantity of both velocities is so great that their sum or their difference may be considered infinitely small in respect to the space it has to pass through; but in comparison with the momentary action of the explosion, the high numbers of the velocities lose nothing of their value as a constant quantity, and there is sufficient time to admit of the rupture of the material, which would have yielded by its own elasticity had the ratio been less sudden in its origin.*

A good illustration of this law is the well known fact that a ball shot from a rifle will pass through a pane of glass without splintering it: but a very striking illustration of the case at present before us is the passing of a cannon

* The well known physical law, that in the communication of motion, and in the generation and destruction of momentum, time must be consumed, and that the nature of the effects produced will depend on the time which is consumed, or on the period of duration of a given action, is a law or ascertained fact which has been but little regarded in comparison with its great importance.

ball through a board, leaving only a very small opening in comparison with the diameter of the striking sphere. The ball breaks only the part of primary contact; then the compression of the wood begins from the centre of contact to the equator of the ball, and then the elastic force of the wood closes the opening again, preventing in its return the further spreading of the force.

The following experiments give a further proof of these circumstances:—If we ignite a grain of gunpowder on the scale of a very sensible balance, accurately counterpoised by a grain weight, a slight motion will be first communicated to the scale of the balance in the opposite direction in which it is thrown by the counterbalancing weight. If, on the contrary, we explode a grain of chloride of nitrogen on the same scale of the balance, a hole is broken at the instant of the explosion through the scale where the chloride lay, and the balance follows undisturbed the action of the counterpoise.

As a small yielding or elongation of the boiler plates in a direction parallel to their sides would prevent all explosions of this kind, I endeavoured to ascertain by experiments as near as possible, and on a sufficiently large scale, the quantity of elongation which would take place before giving way to a breaking force suddenly applied.

An iron wire four feet in length was selected for the experiment: it was to be fixed vertically, and the breaking force applied to its lower end. In order to accomplish this object, I enclosed four inches of its upper end between two steel brackets fitted to the diameter of the wire, and secured the whole as firmly as possible between the jaws of a large vice which was fixed horizontally on a solid vertical stone pillar so that only three inches distance was left betwixt the wire and the pillar. The lower end of the wire was now likewise secured in a similar way in the jaws of another vice, after it had been pushed through the slit of a strong cast iron lever of the second order four feet long from the centre of the fulcrum to the centre of the application of the force. The lever rested in this way on the jaws of the lower vice at a distance of three feet from its fulcrum, and one foot from the centre of the power, to which latter a scale was applied, and the whole apparatus counterbalanced so as not to disturb the result. Weights, each ten pounds, were now gradually and successively applied: with 18 cwt. the wire began to stretch; 22 cwt. stretched it ·298 of its entire length, which was three feet clear; and 23·2 cwt. tore the wire asunder.

Other wires of the same sort of iron were now fixed in the same way, only the scales were removed and a parallelopipedon of cast iron was placed on a

strong foundation under the short end of the lever near the jaws of the lower vice, so that when the end of the lever was loaded with 10 cwt. the distance of the lower end of the lever from the cast iron parallelopiped was just the $\frac{1}{16}$th of an inch, as the distance which I allowed it to stretch.

Over the centre of the free end of the lever, where the scales had been applied, was suspended a weight of 81·76 lbs. with a rounded end, at a distance of 7·58 feet, and then suddenly and carefully liberated from its point of suspension. It is evident that by touching in its fall the shorter end of the lever it created a pressure of nearly 22 cwt. on the suspended wire, a weight which, slowly applied, stretched the iron wire, as before mentioned, ·289 of its entire length; but by the cast iron pillar below not more than $\frac{1}{16}$th of an inch was allowed for stretching. Nevertheless, the wire which supported a dead weight of 22 cwt. very well, broke invariably when the same weight was suddenly applied in twelve different experiments. The same effect took place five times in twelve, when the weight was falling only through a space of six feet. The cross fracture of the rod was even, and its whole length was found to be not at all altered, a proof that it had not stretched before it broke.

I do not consider it out of place here to mention some rather unexpected results which I obtained by experiments made on iron rods and railway bars. Two rods of the same size, one of English, close grained, and the other of long fibrous tough iron, were strained with the same weights, applied gradually. Their strength was found to be just in an inverse ratio by changing the mode of application of the load; the fibrous iron, which was found to be strongest under the gradual application of the load, was found to break by the sudden application of the same load. The same circumstance occurred in testing the transverse strength of iron bars: the toughest but open fibrous iron, which was capable of being doubled by a gradual application of the force, broke invariably short when the same load was suddenly applied; on the contrary, a rail of granulated iron, which would bear bending only in a slight degree before it cracked, resisted the same load suddenly applied perfectly well—facts which appear worthy of consideration in selecting or preparing iron for boiler plates, which ought to be chosen with the closest possible grain, as there the sudden action of the rending force is most to be feared.

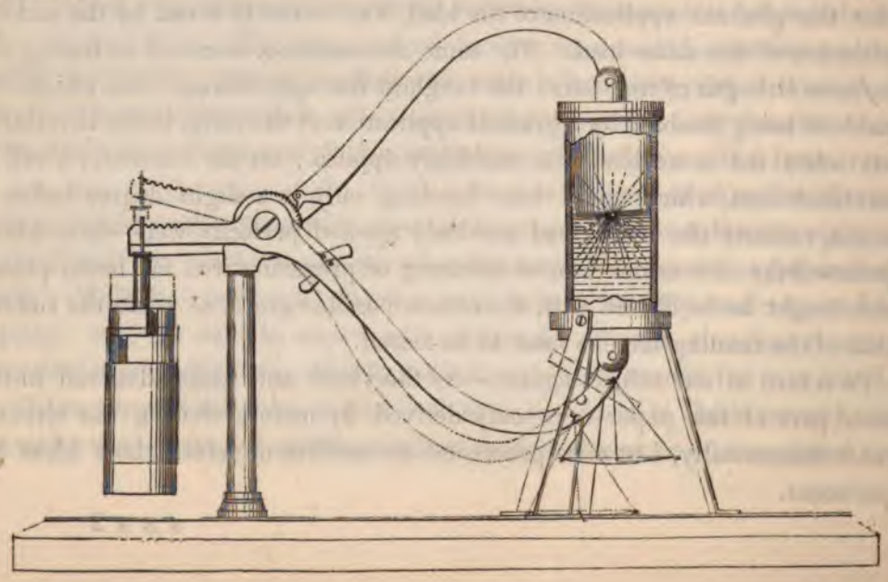
To return to our subject again:—As the views and ideas advanced in the former part of this paper were only derived by merely viewing the circumstances theoretically, I at once proceeded to confirm or refute those ideas by experiment.

Practical confirmation of preceding views.

I placed a hollow cylinder 2½ inches long and about 1 inch internal diameter vertically on three legs three inches long; to the upper and lower end moveable flat lids or plates were fitted air tight, and fixed with their moveable centres to two arms or levers of the first kind, bent in the form of an S, which reached back to a pillar as their fulcrum, which carried a vertical disk whose half circumference was toothed like a ratched wheel. To the centre of this disk and on either side were attached the two arms or S levers, carrying the plates or lids on one end; and near to the ratchet wheel a click, in order to retain both arms at every distance to which they might be thrown off in the direction of the axis of the cylinder. The lower plate or lid, carrying a scale attached underneath to its centre, was now placed so as to close the lower end of the cylinder, into which water was poured to the height of 0·86 of its entire length, and then the opposite end of the lever loaded till one ounce weight in the scale would open the lid slightly and permit the water to escape.

The upper lid, with its lever, was now nicely counterpoised, and the ounce weight from the scale below removed and placed on the upper lid, in order that both lids might be pressed with equal force against the openings. The cylinder, filled to 0·86 of its height with water, resembles in this state a sort of boiler, the top and bottom of which are moveable, yielding to a force exerting the pressure of an ounce: the steam of course was represented by common air.

The following diagram will convey a correct idea of the apparatus above described:—



The next object was to cause an explosion at the surface or at any depth of the water contained in the cylinder, and I readily obtained this object by means of a globule of potassium.

The affinity of potassium for oxygen is the greatest of all known substances. Potassium, therefore, on coming in contact with water, decomposes the latter with great rapidity, developing hydrogen, a process which may be considered in our experiment analogous to the generation of steam from boiling water. But potassium has another property extremely favourable for our purpose. The potassium, after its combustion, is entirely converted into potassa; this potassa remaining in a fused state as a globule, which combines in one instant, developing at once a still higher temperature, with 16 per cent. of water, causing and concluding the combustion with a perfect explosion.

The potassium floats, during its combustion, at the surface of the water. In order to cause its action under water, it must be wrapped loosely in white blotting or filtering paper, and the folds tied with a small piece of lead wire, which sinks it to the bottom, the blotting paper at the same time preventing its too rapid contact with the water.

If we now take a large globule of potassium and throw it into the small cylinder on the surface of the water and immediately shut the lid, the combustion of the potassium and consequently the development of hydrogen, commences immediately; and when the quantity of potassium was large, the upper lid is slightly lifted to let the air and gas escape; but as soon as the combustion is completed and the explosion takes place, the upper lid closes, and the water is forced out through the lower one entirely as the preceding theory predicts.

If we now sink another globule wrapped in blotting paper to the bottom of the water, an explosion ensues as soon as the water penetrates the blotting paper, and water and burning potassium are at once thrown out at the lower end without moving the upper lid at all.

But even a less violent explosive mixture, which developes its power more successively, as gunpowder in an unconfined state, acts in the same way.

I prepared wax paper by placing thin paper on a sheet of metal, heated to the melting point of wax, and rubbing the surface of such paper with a lump of bees' wax until it was entirely saturated with it. From this paper cartridges were formed about half an inch in diameter and about the same length, closed on the one end by folding the paper slightly over it, the joints easily adhering

together by being exposed only to a slight degree of heat. Nine grains of gunpowder were now wrapped in a small bag of writing paper, of a diameter just to fill the cartridge, the folds of which on one end were only slightly twisted together with the fingers, and the bag pressed into the cartridge with its twisted end downwards. A small piece of potassium like a lentil, placed betwixt a small piece of blotting paper, the edges of which were loosely folded, was then placed in the centre of the surface of the gunpowder bag and covered with a wadding of two single disks of filtering paper, a little larger in diameter than the cartridge, and finally the closely filled cartridge covered with another larger disk of blotting paper, the edge of which was bent and folded down the sides of the cartridge and held there firmly by a ring of thin lead wire of 3½ inches long and of 8 grains weight.

The cartridge charged in this manner, weighing 45 grains, when quickly thrown into the water, explodes in about 5 seconds.

The effect of the explosion, when the mouth of the cartridge is kept downwards, is invariably to throw the lower lid from the opening about 1¼ inches, leaving the upper one untouched, although the combustion goes on rather slowly, and a great portion of the gunpowder is forced out without being burnt.

If the bottom of the cartridge is formed of blotting paper only, by bending a disk of blotting paper over a wooden cylinder of the same diameter as the cartridge, and holding the folds forming the sides of it simply by surrounding them with a slip of warm wax paper, this capsule is able to float at the surface of the water. If then a disk of double filtering paper is put on the bottom, (the potassium wrapped slightly in a double piece of blotting paper with folded edges as before, and a capsule of writing paper containing the gunpowder placed immediately upon it,) the gunpowder will be ignited from below whilst the capsule is floating at the surface of the water.

By this means we see remarkably well the progress of the combustion going on with the gunpowder. The gunpowder, without confinement, ignited from below, throws the uppermost grains in the air, where they become ignited, and the whole product of the combustion is deposited on the lid in a beautiful ramified web-like layer. The sides of the cylinder show only traces of this web near the lid. But even in this instance, where the whole combustion goes on entirely in the air chamber and nearest to the lid, the lower lid is at the same instant thrown open and to the same distance as the upper one.

If now, instead of placing the capsule with the gunpowder open upon the potassium, we close this capsule up so as to confine the gunpowder and prevent as much as possible its being thrown into the air before it is thoroughly ignited, the lower lid is generally thrown open twice the distance of the upper one; and in short, the relative distance to which both lids are thrown seems to be somewhat as the distance of the exploding gunpowder from each of them.

The fulminates of gold, silver, and quicksilver, act with still more decisive effect, but the final result of all these experiments is that, under certain circumstances, the exclusive, and in all cases the most powerful, action of the exploding preparation takes place on the lower lid or on the bottom of the cylinder.

Practical conclusions from preceding experiments.

The results of the preceding experiments must, I conceive, be considered in every respect in perfect accordance with the conclusions derived from the theoretical views and calculations already advanced, and I may be permitted to assume that the action due to any sudden increase of pressure will be transmitted through the water and through the steam according to rates at which a wave is transmitted through them respectively. If, then, the sudden increase of pressure arise from the sudden generation of steam near the sides or bottom of a boiler, as on the exposure of a red hot portion of the plates to the water, or from any other cause which may produce such sudden increase of pressure, the bottom or sides of the boiler will receive the action consequent thereon before the safety valve, and thus a boiler may explode before the action which causes that explosion has had time to be transmitted to the safety valve. It follows, also, from the preceding views, that a boiler must receive a succession of these transmitted actions or shocks, and though the first may not have been sufficient to burst the boiler, any shock occurring subsequently in order of time at another part of the boiler may produce that effect. Also, the bursting may, as it were, be commenced by the first and completed by the subsequent shocks, which will be successively experienced at other parts of the boiler. I conceive that these successive shocks may in general be reduced to two, the one, the action which is exerted at the bottom of the boiler, or that part of the sides immediately contiguous to the point at which the sudden action is at first produced; the other, the action which takes place subsequently and almost immediately after on the upper parts of the boiler in contact with the steam, or at the top of the steam chamber.

In addition to the causes of sudden actions already mentioned, are others of constant occurrence and of too much importance to be omitted altogether in this communication.

The sudden opening and shutting of the safety valve will cause a sudden action at every part of the boiler, which, under certain conditions, may be highly dangerous. Any increase in the intensity of the fires at particular parts of the boiler may be attended with similar consequences; and, in fact, any of the various causes which occasion sudden starts of the steam in a boiler, and which frequently arise from the saturation of the water with certain earthly salts, as sulphate of lime, silicia, and alumina.

The causes last mentioned cannot be conceived likely to lead to the disastrous consequence of an explosion so long as the boiler is unimpaired by use. But the rapidity and amount of the change which takes place in the plates of a boiler, subject to the continuous action of the fire, is of too much importance, as the following cases will show, to be passed over entirely without remark on the present occasion.

On inspecting the edges of the fracture of a boiler which had exploded with considerable force, I found that a part of the plate which had been red hot had undergone a remarkable change. A slip of the upper part of the boiler, 1 inch in breadth, was bent to nearly a right angle, by applying a weight of 3 cwt., whilst a slip, exactly of the same size, cut out of the part which had been red hot, broke by an application of 2 cwt.

The fractured end of this slip was cut off and polished with a fine file; and on being carefully examined, exhibited, to a considerable depth, a combination with sulphur.

On a careful inspection of various fragments of other steam boilers which had burst, I found the fractured edges always changed in a similar manner; and I found that the edges of a piece of iron, which was shown to me as a fragment of the boiler which burst on board the Hull steamer, were so brittle that a slight stroke of the hammer was sufficient to break them off; and, when put into a retort and diluted in chlorydic acid, a great quantity of sulphuretted hydrogen was perceptible, the exact quantity of which I was enabled to ascertain by collecting it in a solution of nitrate of lead.

By this experiment it appears to me clearly demonstrated, that the iron plates of a boiler which, by the pressure of steam contained in them, are always kept in a certain degree of extension by the flame acting upon them

(especially by the flame of pit-coal, which always contains a quantity of sulphureous acid,) are in time considerably weakened, as the texture of the iron by this means becomes loosened; and the more pyrites the coal contains the worse it will be, particularly when the boiler, by the sinking of the water level, is often allowed to get into a highly heated or an incandescent state, and afterwards, by the rising of the level, cooled again.

The expansive force of the steam always tends to separate the fibres of the iron, which, in addition, is kept in a state of expansion by the caloric which it imbibes by being allowed to get into an over heated or incandescent state. Afterwards the irresistible contraction of the doubly-expanded metal, caused by its coming in contact with the water, must act upon it in a very powerful and detrimental manner. The slightest irregularity in the generation of steam, which occasions its developement by sudden starts, must naturally tend to burst such a weakened boiler, whether the sides be at the time cool or in a state of incandescence.

Conclusion.

The considerations and experiments contained in this communication open a somewhat new field of practical research; and, as it would appear that the actions on the bottom of the boiler, or on those parts in contact with the water, are the first in order of time, the importance and practicability of obviating these effects is naturally suggested. It has occurred to me, whether some additional security might not be obtained by a valve properly weighted and placed at the bottom or lower parts of a boiler. There are, however, various considerations of importance in the placing and construction of such a valve, and I content myself on the present occasion with suggesting the utility and practicability of such a contrivance as a means of affording some security from the consequences of sudden action arising from the causes which have been mentioned.

CHARLES SCHAFHAEUTL.

London, March, 1840.

XX.—*Remarks on the Duty of the Steam Engines employed in the Mines of Cornwall at different periods.*

By JOHN S. ENYS, ESQ., Assoc. Inst. C.E.*

Read January 14, 1840.

THE advantage of an intimate union of theoretical and practical knowledge in the application of steam power will be readily admitted, and the certainty, that in case of difference of opinion the latter ought to be relied on, should lead us to a cautious investigation of the conditions which may influence the results: it will be the object of my remarks, which embrace only a portion of this subject, to further this union by an attempt to trace the progress of improvement in Cornwall, and to point out some of the principal causes by which it appears to have been effected; the inquiry may perhaps be found deserving of attention, since extravagant and undefined ideas have been prevalent in Cornwall of an almost unlimited increase of duty, while elsewhere the reported duty has been sometimes treated as incredible and inconsistent with the laws of Nature.

I would observe in the first place that no change has taken place in the use of the term duty, or in the principles of its calculation, since its introduction by Watt, yet it may have been materially influenced by causes unconnected with the engine, viz., by the reduction of pitwork friction, and the greater average depth of the shafts from which the water is drawn; the advantage due to the former cause is however in some degree counterbalanced by the improvement

* The author, in compliance with the regulations of the Institution of Civil Engineers, soon after his admission as an Associate forwarded Remarks on Cornish Engines to that Society, an abstract of which was read in January, 1840, and published in the Minutes of that Session. At this period it should be remembered that a duty of 70 millions had been discredited, and the usual proofs had been treated as inconclusive by many persons.

During the last two years, several publications and numerous discussions have rendered this subject more familiar to the public, and the author's opinions on several points have undergone some modification, so that when he was requested, in October, 1841, to revise the paper for publication, it seemed most advisable that notes in explanation should be added.

of the actual, in proportion to the calculated, delivery of water, arising from the better workmanship of the pitwork, and the greater attention paid to the state of its repair by the managers of the mines under whose care it is placed to the exclusion of the engineers, to whom the construction and care of the steam engines are entrusted.

In calculations necessary for the comparison of the power and work performed by steam, it will be convenient to estimate the force and space of action of the steam on the piston in terms similar to those which have been so long employed for duty, viz., of lbs. raised one foot high per bushel of coal, but care must be taken in reference to the older duty, to raise it to the present standard of the imperial bushel of 94 lbs. of Welsh coal; an addition of 5 per cent. to the duty of the Winchester bushel, is an approximation, I believe, in favour of the old engines.

In 1827 Mr. Davies Gilbert, in the Philosophical Transactions, and Mr. Tredgold, in his work on Steam Engines, published in the same year, brought forward independent investigations respecting the power to be derived from steam, and both parties adopted the unit of one cubic foot of water; the former, in reference to the Cornish idea of work performed, estimated the weight of water that would rise 34 feet into a perfect vacuum, assumed to have been formed by the condensation of steam of atmospheric pressure, produced from one cubic foot of water. Tredgold, on the contrary, on the engineer's view of power, calculated the power that could be exerted on a piston by the volume of steam of atmospheric strength formed from one cubic foot of water, obtained by multiplying together the pressure per square foot, and the space through which it could act. The results will be similar if the same volumes of steam are assumed—yet the method of Tredgold seems preferable, since it can be extended to the steam power that may be derived from steam of any pressure, as shown in the Report of the Cornwall Polytechnic Society for the year 1835.

Annexed is a corrected table of pressures translated from the French tables of Clement Desormes, in which the French estimate of the atmosphere is taken at 29·92 inches of mercury.

Obstacles to the correct application of Theory.

The obstacle to the general application of the correct method of expansion pressure calculations, appears to have been the attempt to use the pressure in the boiler instead of the pressure in the cylinder at the closing of the steam valve, as the basis of calculations of the gross power supplied at each stroke by the boilers, and of the gross

power exerted on the piston, which differs from the former wherever expansion is used.

In engines not worked expansively, the boiler's supply is a measure of the steam power—but in expansive engines an addition must be made of the power derived from the steam after the communication between the cylinder and boiler has been closed.

A practice has been also adopted among engineers of deducting the resistances arising from the imperfect vacuum from the boiler pressure, and calling the remainder the gross power on the piston, and of occasionally calculating the expansion pressures at different parts of the stroke from this erroneous basis: this seems to have arisen from a reference to indicator diagrams of double acting engines, in which the difference of the pressure on the two sides of the piston is shown.

The errors due to the belief that the consumption of fuel was proportional to the temperature, which at one time led to the expectation of an enormous advantage from the use of high steam, may be considered as entirely exploded, and the necessity of the introduction of the wasted atmosphere in comparative estimates, where steam of different pressure is used, is also acknowledged. The atmosphere wasted in high pressure engines obviously bears a different ratio to the pressure as higher steam is employed, and the loss theoretically becomes less; in practice we can prevent escape of heat by perfect clothing of the boilers and engines.

I shall now endeavour to show that the Cornish engines are worked under such conditions that a large proportion of the advantage of the expansive action of the steam is rendered available on the piston; but previous to any remarks on this point, I am bound to advert to the corrections which are required in the application to Cornish engines of Boyle's and Marriotte's law, that the pressures of elastic fluids are inversely as the volumes, when the temperature is constant.

Correction of the deficiency of Water in high Steam.

In the first place, the temperature is not constant, but decreases in an unknown ratio, as there is a deficiency of water in a given quantity of high steam to produce another quantity of lower steam of a density exactly proportional to the volumes of expansion—yet for practical purposes the pressures may be assumed proportional to the water at the end of each volume of expansion, unless the temperature is raised by the effect of the correction due to the steam jacket usually employed, containing steam of 270° Fahrenheit,

Correction for Steam Jacket.

while the steam in the cylinder would vary during five times expansion of two and a half atmosphere steam from 260° Fahrenheit to 180° Fahrenheit.*

This jacket steam has also a tendency to produce an increase of pressure by the evaporation of water due to priming, or the admission of water mixed with steam.

Assuming that steam of $2\frac{1}{2}$ atmospheres pressure is nominally expanded to $\frac{1}{2}$ an atmosphere at the end of the stroke, we should have about 593 grains of water at the end of the stroke according to the tables annexed to this paper; but this quantity would afford only 119 grains to each cubic foot of expanded steam—now steam of $\frac{1}{2}$ atmosphere pressure requires 135 grains for its formation; the deficiency of 16 grains is $\frac{1}{8}$th, but as it is probable the deficiency of pressure will be found to be in a similar ratio to the deficiency of water in the expanded steam, capable of affording the full density due to its nominal expansion, the average loss of pressure during 5 times expansion would be about $\frac{1}{20}$th.

It has been stated to me, on authority on which I place confidence, that extended trials of the steam jacket at Wheal Alfred by Woolf, afforded grounds for the belief that the advantage of the steam jacket amounted to 10 per cent., and Mr. Wicksteed has expressed on a recorded trial a similar opinion. Under these circumstances I conceive I am justified in balancing these corrections, and in using the common theory in expansion calculations, believing that the limits of error will not exert too much influence in the comparison with reported duty.

In mining engines, the boiler at each stroke supplies a quantity of steam to fill the space through which the piston has been moved, until the steam valve is closed, and also a smaller portion to raise the clearance steam compressed by the preceding stroke to an equality with that employed to move the piston.†

* The heat expended in the jacket is exclusive of that employed in producing the evaporation. The water condensed in the steam jacket is always in Cornwall returned to the boilers, but at least $\frac{1}{3}$ths of the heat expended during its conversion into steam has been absorbed; the larger portion of this heat is probably communicated through the cylinder to the steam during expansion when its temperature is so much reduced, and tends to keep up its pressure—but to what extent the most accurate experiments alone can determine.

The jacket steam is considered of value in keeping the top and bottom of the cylinder at an equal temperature, but there seems no direct proof in favour of this opinion.

† The words compressed steam refer to the appearances shown by indicator diagrams toward the end of the out-door stroke. When the equilibrium valve is freely opened, the unbalanced weight of rods produces out-door velocity, which is checked and brought to rest by the increase of the steam pressure above the piston, acting through a given space by resistance to compression. In case an engine, or rather the pitwork, is out of balance, or the water in fork, and the pumps are not full, either the equilibrium valve connot be opened freely, or it must be closed sooner.

This clearance steam exerts no power, while the communication between the cylinder and valve is open, but acts during expansion. The complex action is too important to be neglected in expansion calculations, and its effect increases with the amount of expansion. The method I have usually adopted is to measure the portion of the strokes during which the steam valve is open, as the unit of expansion, and to add to the full pressure steam the advantage due to the Hyp Log of the amount of the expansion on the common theory, together with the value of the clearance steam, or to obtain the centre pressure of each volume of expansion, and increase it by the value of the clearance steam.* Perhaps the more correct method would be to calculate the full pressure as before, and then add the clearance steam, before the amount of expansion was calculated; but in this method the expansion shown by the indicator, and the actual expansion in the cylinder do not coincide. When the pressures are all calculated by the centre pressure of each volume of expansion as in the second method, the values of each portion of the steam may be clearly shown at any point of the stroke, and a means is thus obtained of comparing theory with the diagrams formed by the piston of an indicator. For this purpose, it is obviously a point of extreme importance that the pressure in the cylinder, while the communication with the boiler is open, should be ascertained with as much accuracy as the case admits of.

Methods of ascertaining the Steam Pressure in the Cylinder.

Woolf employed a mercury gauge, but the results have not been recorded in a satisfactory manner, and the diagrams made by him, with an indicator that I have recently used, have been lost or mislaid.

In 1835 Mr. Smith visited Cornwall to inquire, on behalf of Mr. Fairbairn of Manchester, respecting the reported duty of the mining engines, and I am indebted to his kindness for a copy of the diagrams then taken, which coincide with the diagrams subsequently taken by the same instrument in January 1838, at Davey's and Hocking's engines by Mr. Loam, the engineer of the Consolidated and United Mines, to whom it was subsequently presented.

I propose to advert only to these diagrams,† in which the coincidence of the

* See Tables at the end of the Paper.

† It has not been considered necessary to insert the diagrams of these engines. The steam was cut off in Davey's engine, at $\frac{1}{8}$th of the stroke, and in Hocking's engine at $\frac{1}{7}$th of the stroke; results which I have since seen confirmed.

The straight line of pressure while the steam valve was open was well defined.

calculated expansion pressures, and the actual pressures as shown by an indicator are more apparent.

The duty of these engines averaged about 70 millions, the column of water on Davey's shaft is 286 fathoms, and above 200 fathoms at the United Mines, at Hocking's engines, and the load on the piston were respectively 13·12 lbs., and 12·7 lbs. per square inch.

The steam valves were rapidly closed, so that the commencement of the expansive action seemed better defined than on the diagrams taken by Mr. Henwood in 1831, in which a convex curve occurs; in those cases I apprehend either the steam valves were more slowly closed, or the clearance spaces were larger, or the full pressure steam was partially wire-drawn during its admission.*

* I do not exactly remember the view I entertained at the period respecting the wire-drawing the steam during its admission from the boiler into the cylinder, the expression refers to the indicator's diagrams taken by Mr. Henwood in 1831 at Huel Towan, the steam valves of which engine was, I understand, $\frac{1}{100}$th of the cylinder area. A double beat valve of 8 inch diameter on an 80 inch engine I conceive is unable to supply the steam with sufficient rapidity after the piston has attained velocity; the result is a partial expansion before the communication between the boiler and cylinder is closed, and a convex curve of pressures is shown by the indicator.

In the engines at Consols, and at the United Mines, 12 and 13 inch steam valves are used, and the area is about $\frac{1}{45}$th of the cylinder; and several indicators showed that while the steam valve remained open, steam of the same pressure was admitted into the cylinder, and consequently the expansion curve assumed a concave form, and the actual coincided more closely with the nominal expansion, and with a calculated curve of expansion including the clearance.

However, the duty of several engines with 8 inch valves has exceeded that performed by Davey's, at Consols, and Hocking's engine at the United Mines.

It is possible there may be an advantage in this method from a pressure gained from high steam in the boiler, without any expenditure of water; but whether this plan is preferable to the introduction of equal quantity of water at an equal pressure of steam while the steam valve is open is an undetermined point which involves questions of strength of boilers and cylinders in a manner such as not to admit of an easy solution.

Recently at an engine (Taylor's United Mines) in which a 14 inch steam valve is used for an 85 inch cylinder, several indicators have shown that the steam of the same pressure, while the valve is open, viz., about 25 lbs. above atmosphere, is admitted, and expanded about 12 times; this engine has performed a duty of 100 millions for three months in succession.

The variation of the pressure at the top and bottom of the stroke, in consequence of the proportional greater effect of the clearance steam during expansion, seems to be about as 8 to 1.

Woolf once attempted to work a 90 inch cylinder with a 4 inch valve; under these circumstances the velocity of the steam through the aperture would approach the limit of its velocity. A duty of 44 millions was the result, but it was subsequently increased to 67 by means of an 8 inch valve. The use of small valves may be traced to Woolf's practice.

Mr. Henwood's most valuable series of indicator diagrams have been published in the Transactions of the Institution, vol. ii., page 49, and are the earliest on record.

It may perhaps be questioned whether the spring of an indicator can follow with sufficient rapidity the decreasing steam pressure during expansion; whether it has not a tendency to exhibit a pressure higher than is exerted by the steam on the surface of the piston, occasioned by the small passage of communication through a wooden plug fitted into a small grease cock, &c.; still I conceive it may be relied on sufficiently for the steam when the valve is open, at least for those engines in which the full pressure steam exhibits a straight line, from which calculations may be made of the gross power capable of the performance of a duty equal to that reported in 1838.*

With a view of showing the progress of improvement, and its connection with the results that may be deduced from theory, it is proposed to select the duty or work performed in the years 1778, 1798, and 1838, as representing

* During the spring I had an indicator constructed with a small piston, under the impression that it might give more accurate results from steam admitted only through the small aperture, a wooden plug was inserted into a small grease cock. I had reason to believe it was properly adjusted for low temperatures, as on a trial at Halen Beagle, on an engine loaded only to 5·76 lbs. to the square inch of it, together with the Consol's indicator (made by Woolf), both indicators showed that a decreasing steam pressure crossed the atmospheric line while the steam valve was open. On opening the grease cock at the same moment as the steam valve, a very small puff of steam was visible, and the engine audibly took air before the steam valve closed, some proof of the accuracy of the indicator under these circumstances; but when the smaller indicator was placed on Taylor's engines, where the temperature of the steam was much higher, the first stroke when cold gave a much higher result on the scale than when heated; but the scale when heated coincided with the Manchester indicator, which belonged to Mr. Loam, one of the engineers of the mine. A similar result again occurred after correction by the maker.

I mention this circumstance, as I wish to retract, in some degree, the strong expressions I had used as to the dependence that may be placed on indicators without extreme attention to accuracy of adjustment, as well as to the channel of communication with the cylinder.

I may, perhaps, be permitted to advert to another necessary precaution in their use: several diagrams were recently taken, with this and Woolf's indicator, at Tresavean, with a string from the radius rod, which in this case made rather a greater angle than usual with one of the grease cocks, and also with a parallel motion introduced between the radius rod and indicator, which was fixed by a temporary frame of boards. The average result of the diagrams from the string was an expansion of $5\frac{1}{2}$, and with the parallel motion of $5\frac{1}{4}$ times, the difference was about $\frac{1}{10}$th of a foot in the cylinder, or 1·2 of the point at which the steam valve was closed.

The engine is similar to that at the United Mines, except that the stroke is 12 instead of 11 feet: it had scarcely been at work 3 weeks. The expansion will be soon much increased.

the performances in Cornwall of Newcomen's atmospheric engine, Watt's low pressure engine, and Watt's engine working high steam expansively.

The duty is a calculated quantity which has at all times been affected by numerous causes, exclusive of the changes that have taken place in the boilers and pitwork, which will be adverted to at the end of this paper.

The annexed list was published in the Appendix to the Ordnance Geological Survey of Cornwall, in reference to water raised from the mines :—

" At all periods, the friction in the pumps on a given load has varied in different shafts from several causes, among which may be enumerated the original quality of the workmanship of the pitwork, the state of order in which it is kept, the length and number of the lifts, the diameter of the pumps, the underlie of the shaft, and the use of flat rods to work a second column of water in another shaft.

"The delivery of the actual water raised, as compared with that calculated has always varied from the impracticability of at all times supplying the exact quantity of water, when pumps of different diameter are employed at different lifts, especially when part of the water is taken from the upper levels, which are liable to be affected by rain in winter, or where the pumps are worked in shafts of unequal depth, independently of leakages of all kinds; all these circumstances have been neglected for the sake of a convenient method of comparing the work performed by steam engines with different velocities, and loads within certain limits, a course rendered necessary by the nature of the agreement between Messrs. Boulton and Watt and the Mining Adventurers, and, since 1812, by the spirited competition that had been established among the mining engineers by monthly reports of the duty of the Cornish engines."

It appears to me almost impracticable to attempt, in contracted shafts, to prove the duty in the manner often demanded of weighing the water delivered by each separate lift, without which precaution the real duty could not be exactly ascertained, the adit delivery is unsuited even for an approximation, as it includes the water drained from the upper portions of the lode; the objections above enumerated are less felt in new pitwork, such as is attached to the engines adverted to in these remarks.

Water Delivery.

The earliest attempt to ascertain the water delivered was made by Mr. Henwood, at Huel Towan, who estimated the deficiency at 7 or 8 per cent; subsequently Mr. Wicksteed weighed and measured the water from three lifts of pumps, at Holmbush, and found the deficiency 10

per cent. In September, 1839, the water of four strokes of Eldon's engine was separated and measured out of a cistern, the plunger was 14 inches in diameter, the stroke 7½ feet, and the lift 34 fathoms; the calculated contents of the pump was 49 gallons, the delivery 47, a deficiency of 4 per cent.

The position of this pump near a road, over which I constantly passed, enabled me to watch the performances, and in course of 30 visits or more, I never but once detected an air bubble, and even then the amount was scarcely appreciable—less than a cubic inch.

Summary of Improvement.

It will perhaps be found a convenient form to exhibit a short summary of the principal changes that have taken place during 60 years, in connection with the amount of duty performed at the periods selected:—

		Duty.
Newcomen's engine, 1778.	Dome boilers under the cylinder.	
	Separate pump rods for each lift.	
	Bucket pumps.	
	Full load for atmospheric pressure, probably 7 or 8 lbs. per square inch.	
	Column of water seldom exceeding 90 fathoms.	
	Trial at Poldice of two engines in one shaft	7,126,822 Winchester. 7,483,163 Imperial.
	Average of other engines under	5,000,000 Winchester.
	Evaporation of water . .	6 or 7 cubic feet.
1798. Watt's low pressure engine.	Waggon boilers.	
	Cylinder placed on a separate foundation.	
	A connected main rod, with pump rods attached to each lift.	
	Bucket pumps.	
	Loads generally too heavy for much expansion with low pressure steam.	
	Duty of 4 best engines	27,473,500 Winchester. 28,847,170 Imperial.
	Average of 23 ditto .	16,925,000 Winchester. 17,770,000 Imperial.
	Column of water seldom exceeding 160 fathoms.	
	Evaporation of water from 9 to 10 cubic feet.	

1838. Watt's engine, using high steam expansively.	Trevithick's boilers. Main rods as before. Plunger pumps, except at the bottom of the shafts. Loads from 5 to 18 lbs. per square inch. Expansive action from 2 to 6 times the volume used at full pressure. Column of water in one instance 286 fathoms. Evaporation of water from 10 to 14 cubic feet.

Before adverting to the theoretical estimates due to the assumed water evaporation of the different periods, I would observe that the Cornish system of mining accounts, in which no reference is made to the capital expended, has afforded the mining engineers more liberty in the adoption of whatever proportions appeared advantageous in the boiler surface in the flues, or in the size of the cylinder for expansion, and in an increase of strength of the pitwork, required for the first impact of high steam, on commencing motion at each stroke with a pressure that so much exceeds the total resistance, while the resistance exceeds the power in a greater ratio at the end of the stroke.

On the commencement of the present system of encouragement of an improvement of duty, the weakness of the waggon boiler was soon found by Messrs. Jeffree and Gribble, at Dolcoath, to be the limit of expansion and to an increase of duty, much beyond that which had been performed by Watt; but on the adoption of Woolf's cast-iron tubular boilers and a modification of Trevithick's locomotive boilers the strength of the pitwork became the limit to expansion,* and recently in some cases the low rate of duty has been traced to weak pitwork.

* The increasing depth of the shafts, and the greater strain occasioned by high steam at the commencement of motion at each stroke, required a greater strength of main rods, and consequently timber of larger dimensions, and a greater weight of iron connecting straps, on a given water load; these increased weights enabled expansion to be again extended, as the means of equalizing the variable action of the steam became more efficient. This mode of meeting the difficulty arising from the variable pressure of steam worked expansively is due rather to the circumstances under which the mines were worked than to design on the part of the engineers. Mr. Davies Gilbert, I believe, was the first person who fully understood the principle on which the success of expansion depended, and foretold the failure of a scheme suggested by a gentleman of Penzance, who attempted to introduce a scheme of the same nature as the patented contrivances of Watt, for the purpose of equalizing the action of steam during expansion. The inconvenience and expense of breakages during the progress of expansion was experienced in all the deeper mines, and in none more so than in the United Mines, which,

As it is my object only to deduce a probable duty from the conditions under which the steam is assumed to have been worked in the cylinder by means of a co-efficient, it seems unnecessary to enter upon the question of total resistances of mining engines* (as the subject would require more information

which, about 1814, were about 170 or 180 fathoms in depth, and the levels were below those at Consols, which are situated to the north, and the mine consequently was over pressed with water.

I have recently received an account of the expense of these breakages from a gentleman through whose hands the payments passed, and whose opinion was strongly in favour of strong main rods for mining to meet the strain occasioned by deepening shafts and increased expansion. The difference of the strain on the main rods with 12 times expansion in Watt's engines, or in Woolf's, or rather Hornblow's double cylinder engine, and also in the double cylinder engine recently erected by Sims at Carn Brea, ought to become a subject of consideration to the miner. It may, however, be observed that the only friction due to the additional weight, of rods and balance, is that occasioned by the additional friction thrown on the gudgeons of the balance beams; the cost of the repairs of which would be scarcely appreciable, while the practice of not charging the interest on capital expended in mining machinery in Cornwall renders a commercial comparison impracticable with the amount of annual coal savings.

* The evasion of this question in 1839 arose from the author's inability to assign sufficient reasons for his belief that the friction and other resistances of good mining engines, even in the deep shaft, did not exceed $\frac{1}{4}$ the calculated water-load, or rather that the addition of $\frac{1}{2}$ to the calculated water-load, would represent the total resistances. This idea had been derived from the calculations attached to the diagram forwarded to him by Mr. Fairbairn, and without doubt it has influenced the co-efficient of that he employed for engine and pitwork resistances.

This rule, however, gave a friction much too high for Eldon's Engine United Mines, a 30 inch cylinder employed in raising injection water in a one plunger lift of 34 fathoms, conditions equivalent to those under which the Cornish Engine at Old Ford is worked. Lately the author, on the suggestion of Mr. Parkes, attached a mining gauge to the following engines :—

1841	—	Cylinder in Inches.	Load in lbs. per Square Inch.	One-half added for resistances.	Barometer.	Atmosphere in lbs., 49 lbs. per Inch of Mercury.	Steam above or below.	Total Steam per Square Inch.	Addition for Atmosphere on Piston Rod.	Total pressure in-doors.
16 April .	North Roskear	70	9·54	14·31	29·4	14·4	=0	14·4	·12	14·52
4 May .	Halen Beagle.	70	5·7	8·55	29·56	14·48	−5·5	8·93	·12	9·05
13 Oct. .	Tresavean .	85	11·91	17·91	29·5	14·45	+3	17·45	·11	17·56

The steam valve was opened by a hand lever, and the velocity in-doors was extremely slow at North Roskear and Halen Beagle, and the mercury was quite steady; but it amounted to about $\frac{1}{2}$ the usual velocity at Tresavean, and 3 lbs. is taken as the mean between the variations of 1 and 4, as the latter was the pressure for the longer time. The pressure seemed less in the subsequent strokes, when the engine came in-doors slower.

The water load per square inch on the piston in Cornwall is usually calculated from the diameter of the cylinder.

than I possess, and repeated visits to pitwork under ground), and the duty has been taken $\frac{6}{10}$ths, leaving $\frac{4}{10}$ths for friction and other resistances of the calculated power.

It is essential to obtain as near as may be the evaporation of water in cubic feet per bushel of coal, and it is to be regretted that scarcely any attention has been paid to this part of the subject by the engineers of the mines of Cornwall.

The careful experiments of Smeaton cannot be taken as the evaporation due to Cornish practice in 1778, nor can the careless waste of the collieries become our guide. I conceive it probable that it did not exceed 6 cubic feet per bushel in dome boilers.

In this year a trial of two atmospheric engines working in one shaft was made at Poldice (Phil. Trans. 1830), under the superintendence of Messrs. Boulton and Watt, and several gentlemen connected with the mines of the county, when the consumption of coal amounted to 14,080 Winchester bushels in 61 days, and hence each bushel, without allowance for stoppages, worked the engine 6·2385 minutes. The four lifts of pumps were 17 inches in diameter, and raised the water from a depth of 58 fathoms, and made $5\frac{1}{2}$ strokes of 6 feet in length per minute.

$$17^2 \times \overset{\text{lbs.}}{2{\cdot}0454} \times \overset{\text{fms.}}{58} = 34{,}285 \text{ load on the shaft.}$$

$$\overset{\text{ft.}}{6} \times \overset{\text{stro.}}{5\tfrac{1}{2}} \times \overset{\text{min.}}{6{\cdot}2385} = 207{\cdot}87 \text{ feet per minute.}$$

$$\overset{\text{lbs.}}{34{,}285} \times \overset{\text{ft.}}{207{\cdot}87} = 7{,}126{,}832 \text{ lbs. raised one foot high.}$$

This amount was subsequently deducted from the duty of Watt's engine, who was entitled by the conditions demanded for the use of his patent to receive $\frac{1}{3}$rd of the value of the remainder, which represented the coal saving effected by his engines. It may be considered a singular circumstance that the neglect and want of attention and skill produced as great a waste of coal in mining engines, after 1800, as the payments made to Watt; and it may be fairly asserted that Watt's counter to ascertain the duty saved nearly the whole amount of the 170,000*l.* paid to Messrs. Boulton and Watt for coal savings. A portion of this sum arose from the increased duty occasioned by boiler and pitwork improvements, unconnected with the patent.

In 1793 the duty averaged on Watt's authority equalled $19\frac{1}{2}$ millions.

In August, 1811, the duty of several engines equalled $13\frac{1}{2}$ millions, being a difference of 6 millions.

Boulton and Watt's share on 19½ millions would be—

$$\frac{19{\cdot}5-7}{3}=\frac{12{\cdot}5}{3}=4{\cdot}166 \text{ duty to be paid as Watt's share,}$$

$$\text{and } \frac{4{\cdot}166}{19{\cdot}5}={\cdot}213 \text{ of the value of the coal consumed.}$$

The evaporation of Watt's boilers is stated on authority to have been from 8 to 12 cubic feet of water per bushel; but from complaints of the carelessness of enginemen, the frequent stoppages of the pitwork at that period, and waste of steam, and perhaps the practice of allowing the enginemen to take home cinders, would induce me not to rate the evaporation at more than 9 cubic feet per Winchester bushel, expended in the cylinder in mining engines.

The present evaporation of Trevithick's boilers is supposed to be from 10 to 14 cubic feet per imperial bushel (94 lbs.), and even more occasionally, an amount due probably to slow combustion and a large extent of flue surface.* In March, 1826, a trial of 4½ days' duration at Woolf's engine, a single-acting 90-inch cylinder at Consols, gave an average result of 11·4 cubic feet per Winchester bushel, in boilers with a less flue surface than is at present employed. The water supply was measured by a cistern, the temperature of which was 90° F., and the temperature of the boiler 263·5 F.

Days.	Hours.	Cubic Feet.	
1	14	12	
2	24	11·34	
3	24	11·64	= 11·4.
4	24	11·11	
5	24	10·93	

This engine first reached 67 millions in the report, and 64 millions on a trial, and exceeded the double cylinder engines in duty, but was soon surpassed by the performance of the Huel Towan, constructed by Grose.

* In 1839 I was an advocate of slow combustion and an extended boiler surface. The Cornish evidence in its favour is founded in a great measure on duty, and it has been mixed up with expansion from high steam, and with the additional steam room given, as more flue surface has been added.

Rapid combustion can scarcely be tried in a satisfactory manner with Trevithick's cylindrical mining boiler, unless a steam reservoir is added equivalent to the steam room of the reduced number of boilers.

The water in many mines is extremely bad, and is supposed to be more injurious to the iron when the fires are forced. This revision of opinion has been occasioned by Mr. Wicksteed's experiments, who has thrown some doubt on this subject in relation even to mines.

The calculated performance of this engine at 5 times expansion—the reported amount—would be—

Temp. 263° F. . .		3,880,000	lbs. one foot from one cubic foot of water.
	$\frac{1}{9}$th −	430,000	for clearance steam.
		3,450,000	
1,609		5,551,050	
$\frac{1}{9}$th +		616,783	for clearance steam.
		9,617,833 × 11·4=108,643,296 × 6=65,859,777.	

A table has been annexed on similar principles of the duty assumed to be due to the different conditions under which the engines were worked in 1778, 1798, and 1838. It is not intended to exhibit any remarkable coincidence where numerous interferences occur, and it is liable to the charge of having been purposely made to coincide, still it affords some grounds for the assertion that the Cornish mining engines ought to perform the reported duties wherever the weight of the main rod is sufficient to absorb the pressure at the commencement, and restore it at the end of the stroke.

In conclusion I propose to advert to some of the changes that have taken place in pitwork during the last 60 years.

Smeaton, in his report on the Chacewater engine, directed a single main rod to be used for the upper lift, and that a cross-head should be attached with rods on each side to work the two lower lifts. The circumstance of a single main rod at the surface was mentioned to me by Mr. M. Moyle, who was an agent in the counting-house of this mine at the period of or soon after its erection. The extension of this plan seems due to Watt or Murdock in lieu of the separate lifts to each pump, with their apparatus of chains at the arc at the end of the beam.

As the economy of coal and greater certainty in the performance of Watt's engine enabled the mines to be worked deeper, the weight of the rod, being in excess, took the engine out of doors too fast without a balance; and either Watt or Murdock (the latter was the party immediately engaged in the suggestion) proposed to substitute a plunger pole for a balance at Huel Maid. In 1796 a plunger pole was employed at Ale and Cakes, a mine which now forms the eastern part of the United Mines.

Under these circumstances no effect of much importance on the duty was produced, but its subsequent introduction as a system of pitwork in lieu of bucket pumps at the bottom of the shaft has eventually caused the slow out-

door stroke. The facility with which the packing can be kept in order, and the trouble of examining and packing common pumps, contributed to its introduction.

Duty of Stamping Engines.

I may perhaps be permitted to refer to an objection which was long urged against the truth of the reported duty of the pumping engines, I mean the low duty of the stamping engines previous to December, 1835, when 43 millions was reported as the performance of a 32-inch single-acting cylinder engine 10 feet stroke, erected by Mr. James Sims at the Charlestown Mines, near St. Austle. In this engine the crank was applied direct to the cam-shaft. The amount of the duty of this engine, and of two similar engines at Huel Kitty and Carn-Brea constructed by Sims, has been increased to above 60 millions. A double engine of similar form at Huel Vor by Richards has equalled this performance.

The expansion of these engines varies from $2\frac{1}{2}$ to 3 times, the steam being cut off at $\frac{6}{10}$ or $\frac{1}{3}$ of the stroke; hence the theoretical duty for 12 cubic feet of water evaporated per bushel scarcely reaches 60 millions for 3 times expansion, a coincidence sufficiently close with the reported duty.

It should not be forgotten that the Cornish duty includes the advantage due to a supposed superiority of Welch coal, and to slow combustion in extended flue surface; still a reference to the first principles of steam power may tend to discourage extravagant expectations, and at the same time afford a reasonable hope that a great improvement in duty may be effected in other districts wherever the conditions may be suitable and high steam would not be considered objectionable.

JOHN S. ENYS.

PER SQUARE INCH.			Temperature Fahrenheit.	Grains of Water in One Cubic Foot	Pressure in lbs. per Square Foot	Volume from one of Water.	Efficiency or Gross Power.
Steam in Atmosphere.	Steam in Inches of Mercury.	Steam in lbs weight.					
·125	3·74	1·83	124·6	37·	264·6	11801·	3,112,544
·25	7·48	3·67	150·8	70·3	529·2	6198·38	3,280,182
·5	14·96	7·35	179·6	135·	1058·4	3229·36	3,427,954
·75	22·44	11·02	197 6	191·8	1587·6	2217·2	3,520,466
1·	29·92	14·7	212·	256·2	2116·8	1700·	3,598,560
1·25	37·5	18·37	223·88	315·	2646·	1384·36	3,664,016
1·5	44·48	22·05	234·32	372·	3175·2	1171·59	3,720,332
1·75	52·36	25·72	241·78	428·8	3704·4	1061.66	3,766,145
2·	59.84	29·4	250·79	485·	4233·6	899·91	3,809,848
2·25	67·32	33·07	257·4	539·6	4762·8	808·	3,848,424
2·5	74·8	36·75	263·93	593·	5292·	733·45	3,881,417
2·75	82·28	40·42	269·89	645·4	5811·2	672·36	3,907,618
3·	89·76	44·1	275·	702·8	6350·4	620·74	3,941,947
3·25	97·18	47·77	278·8	755·6	6879·6	576·83	3,971,359
3 5	104·72	51·44	284·59	808·7	7408·8	539·1	3,994,084
3·76	112·2	54·94	288·86	861·4	7938·	506·15	4,017,818
4·	119·68	58·8	292·91	920·2	8467·2	477·05	4,039,277
4·25	127·1	62·47	295·95	966·	8996·4	450·96	4,046,926
4·5	134·61	66·15	300·47	1017·8	9525·6	428·36	4,080,386
4·75	142·12	69·82	304·07	1071·3	10054·8	406·76	4,098,890
5·	149·6	73·5	307·91	1122·3	10584·	389·38	4,111,118
5·25	157·08	77·17	311·	1144·1	11123·2	372·32	4,141,399
5·5	164·08	80·85	314·06	1221·7	11652·4	356·86	4,157,282
5·75	172·72	84·52	316·94	1271·	12181·6	342·76	4,175,365
6·	179·62	88·2	320·	1322·5	12700·8	329·65	4,186,518
6·25	187·1	91·87	322·77	1373·	13200·	317·58	4,217,462
6·5	194·58	95·55	325·86	1422·	13739·2	306·62	4,218,937
6·75	201·74	98·22	328·71	1473·	14288·4	296·35	4,233,367
7·	209·44	102·9	331·56	1502·	14817·6	286·7	4,243,206
7·25	216 92	106·57	334·19	1570·	15346 8	277·77	4,261,806
7·5	224 4	110·25	336·93	1617·	15876·	269·52	4,278,899
7·75	231·36	113·92	339·38	1666·	16405·2	261·66	4,292,689
8·	239·36	117·6	341·83	1714·	16934·4	254·27	4,305,909
8·25	246·86	121·27	344·22	1762·	17463·6	247·31	4,318,923
8·5	254·42	124·95	346·6	1811·	17992·8	240·76	4,331,946
8·75	261·48	128·65	348·99	1858·	18522·	234 57	4,350,465
9·	269·2	132·3	351·32	1905·	19051·2	228·72	4,357,390
9·25	276·76	135·97	353·62	1953·	19643·4	223·17	4,385,817
9·5	284·16	139·65	355·8	2001·	20109·6	217·89	4,381,680
9·75	291·32	143·22	357·71	2048·	20638·8	212·82	4,392,547
10·	299·2	147·	359·6	2096·	21168·	207·98	4,402,520

These Tables were calculated from those of Clement Desormes, about three years ago.

The efficiency column is original, in its present form, except for steam of 1 atmosphere strength. See Tredgold, on Steam Engines.

EXTRACT from the Second Paper, by Davies Gilbert, Esq., relating to Cornish engines in the Phil. Trans. 1830.—Winchester bushel of Coal.

—	Diameter of Cylinder in Inches.	Duty, 1798.	Observations.
1 .	20	10,015,000	
2 .	21	16,385,000	
3 .	45	29,668,000	It was believed at the time that some inaccuracy must have occurred in the communications respecting these two Engines.
4 .	36	28,212,000	
Double 5 .	42	18,193,000	On the same Mine, the length of strokes in all but one 6 feet, in that 8 feet. Average duty of the whole 15,985,000.
6 .	63	15,190,000	
Double 7 .	45	15,180,000	
8 .	45	15,571,000	
9 .	45	15,090,000	
10 .	45	14,384,000	
Double 11 .	42	18,740,000	
12 .	42	15,532,000	
13 .	36	18,465,000	
14 .	..	12,226,000	The diameter of the cylinder not returned.
15 .	30	14,050,000	
16 .	20	12,366,000	
Double 17 .	14¾	6,097,000	
18 .	30	13,931,000	
19 .	28	19,739,000	
20 .	36	24,514,000	
21 .	21	13,215,000	
22 .	20	15,034,000	
23 .	48	27,500,000	Supposed to be the best Engine.

It is to be regretted, that the load in the shaft, and other particulars from which the duty was calculated in 1798, by Mr. Davies Gilbert, have been mislaid, so that I have been unable to give the statements in the same form as the present duty report, neither have they been preserved by Mr. Alfred Jenkins, who was jointly engaged in making the requisite calculations from data supplied by the different mines.

The duty on Watt's authority was higher in 1793: at this period few new engines were erected, and the shafts had been deepened and a deterioration of the engines had occurred by age or bad water, circumstances tending to reduce duty.

NORTH DOWNS, NEAR REDRUTH.—NEWCOMEN'S ENGINE, with low Steam.

Water in Cubic Feet.	3,600,000 1,800,000 (½) 1,800,000	3,600,000 1,200,000 (⅓) 2,400,000	lbs. one foot high from the water. Waste by condensation. On the piston.—efficiency.
6	10,800,000 6,480,000	14,400,000 8,640,000	Efficiency. Duty.
7½	13,500,000 8,100,000	18,000,000 10,800,000	Efficiency. Duty.
12	21,600,000 12,960,000	28,800,000 17,280,000	Efficiency. Duty.
14	25,200,000 15,120,000	33,600,000 20,160,000	Efficiency. Duty.

WATT'S ENGINE, with low Steam.

—	Full Pressure.	Cut off at Four-fifths.	At Two-thirds.	At One-half.
	3,630,000	3,630,000 733,260 (·202) 4,363,260	3,630,000 1,470,150 (·405) 5,100,150	3,630,000 2,515,590 (·693) 6,145,590
8	29,040,000 17,424,000	34,906,080 20,943,648	40,721,200 24,432,720	49,164,720 29,494,832
10	36,300,000 21,780,000	43,632,600 26,179,560	50,901,500 30,540,900	61,455,900 36,873,540
12	43,560,000 26,116,000	52,359,120 31,415,472	61,081,800 36,658,080	73,747,590 44,248,248
14	50,400,000 30,240,000	61,085,640 36,651,384	71,262,100 42,757,260	86,636,260 51,981,756

WATT'S ENGINE, with high Steam.

	Two and a Half Atmospheres.			Three Atmospheres.	
Expansion.	4.	5.	6.	8.	12.
	3,880,000 388,000 ($\frac{1}{10}$) 3,492,000 4,839,912 (1·386) 483,991 8,816,903	3,880,000 430,000 ($\frac{1}{9}$) 3,450,000 5,551,050 (1·609) 616,783 ($\frac{1}{9}$) 9,617,833	3,880,000 485,000 ($\frac{1}{8}$) 3,395,000 6,078,050 (1·791) 759,756 ($\frac{1}{8}$) 10,232,806	3,940,000 788,000 ($\frac{1}{5}$) 3,152,000 6,453,008 (2·079) 1,291,601 ($\frac{1}{5}$) 10,896,609	3,940,000 985,000 ($\frac{1}{4}$) 2,955,000 6,340,220 (2·484) 1,585,055 ($\frac{1}{4}$) 10,880,275
10	88,169,030 52,801,418	96,178,330 55,704,598	102,232,060 61,396,836	108,966,090 65,379,654	108,802,750 65.281,650
12	106,602,836 63,961,701	118,213,996 70,928,397	122,791,672 73,675,003	130,759,300 78,455,480	130,563,300 78,337,980
14	123,436,642 74,061,985	134,649,662 80,789,797	143,325,928 85,995,556	152,552,315 91,531,389	152,323,850 91,397,310

This Table will account for a great variation of duty in mining engines; on visiting several years ago many engines whose reported duty was low, the principal cause was obviously the small extent of expansion used; the reasons assigned were chiefly weak boilers and weak pitwork: in some cases bad pitwork produced a great amount of extra friction and aided in the reduction of the duty, so that the co-efficient would be erroneous, in other cases.

I believe the co-efficient, 4, will be found to be too high for friction and resistances where the pitwork has been well put up and is nearly new, or during experiments where unlimited quantities of grease are used both in the engine and under ground.

INDEX

TO

VOLUMES I., II., AND III.,

OF THE

TRANSACTIONS OF THE INSTITUTION OF CIVIL ENGINEERS.

	Vol.	Page
Action		
of steam in Cornish engines	iii.	257
expansive and unexpansive of steam, separately valued	„	263
percussive or instantaneous, of steam, and other aëriform fluids	„	409
sudden, suggested cause of explosion of steam-boilers	„	425, 436
See Steam, Cornish Engines, &c.		
Adie, Mr.,		
observations on the openings in walls during severe frost	iii.	202
experiments on expansion of stone	„	*ib.*
Air,		
proper admission of, to furnaces	ii.	162
See Blast, Combustion, Smoke.		
Albano, M. B.		
account of stone bridge over the Dora Riparia, near Turin	i.	183–194
See Bridge.		
Albion Mills Engine,		
experiments on, by Watt and Rennie	iii.	3, 59
particulars of, and boilers (tables)	„	45, 76
economy of, compared with others	„	59, 60
Analysis		
of the ashes of peat coke	iii.	169
of the action of steam in the Cornish engines	„	260
Arches,		
on settlement of (M. Albano)	i.	189
on expansion of, with changes of temperature	iii.	201–218
rise of rib of Southwark bridge	„	203–209
expansion of arch and openings in parapet of Staines bridge	„	214–7
Arch-stones,		
method of restoring, in Blackfriars bridge	i.	181
Artesian Wells,		
on sinking of, and supplying of water from	iii.	229
See Well.		
Atmospheric Engine,		
experiments on Newcomen's, by Smeaton, to determine consumption of coal and steam (Table)	iii.	2, 5, 20 „ 45, 58, 76
the type of Watt's engine	„	58
Auxiliary Steam,		
on the adaptation of, to sailing vessels (Mr. S. Seaward)	iii.	385–408
the limits of the application of steam power	„	386
increased speed consequent on diminished power and weight	„	387–389
instances of combining steam power with sailing	„	391
result of, in the Vernon and Earl of Hardwicke	„	394–399
commercial objections to use of	„	402
application of, to vessels of war	„	403
table of velocities of steam ships with engines of different weights and powers	„	408
Barges,		
experiments on resistance to, in canals (Mr. H. R. Palmer)	i.	165
resistance nearly as the cube of the velocity	„	172
Barlow, T.		
on the thickness of the cylinders of the Bramah or hydraulic press	i.	133–139
determination of law of resistances to barges	„	172
on the power of locomotives at different velocities	iii.	183–196
Barlow, T. P.		
on the strain to which lock-gates are subject	i.	67–80

	Vol.	Page
Berthier, M.,		
estimate of calorific power of charcoal, coke, and peat coke	iii.	167, 172, 177
great value of his litharge test	„	169
Blast,		
application of hot air, in the manufacture of iron	i.	81
the resistance from, in locomotives	iii.	143
duration of jet	„	*ib.*
intermittent action of, explained	„	144–146
experiments on force, and temperature of	„	147
velocity of, assigned	„	152
Bogs,		
upper strata of, more valuable for fuel than lower	iii.	165
See Turf.		
Boilers.		
See Steam Boilers.		
Boring,		
different kinds of, for water	i.	145
under what circumstances to be adopted	„	146
the supply obtained by, limited	„	147
instances of failure, and of interference with other sources	„	143
various cases of, in vicinity of London	„	155
Borthwick, Mr. M. A.,		
on the history of cast-iron piling, and its use at the Brunswick-wharf, Blackwall	i.	195–205
Bourns, Mr. C.,		
on setting out railway curves	iii.	295
See Curves.		
Boyle.		
the application of his law to steam	i.	222
requisite corrections in his law on its application to Cornish engines	iii.	451
Bramah, F.,		
experiments on the strength of cast iron	ii.	113–135
Bridges.		
balance bridge at Hull docks	i.	11
on action of wind on suspension bridges (Col. Pasley)	iii.	220–227
on floating or steam ferry bridge (Mr. Rendel)	ii.	213–225
Blackfriars,		
method of restoring arch-stones in	i.	131
Bow,		
account of construction of (plates)	iii.	343–356
Mr. Burges's history of the old bridge	„	*ib.*
Bridges.		
Calder,		
account of wooden bridge over (plate) (Mr. Bull)	ii.	87
Chester,		
account of construction of (Mr. Trubshaw)	i.	209–214
centre in, and method of striking	„	211
Hammersmith,		
system of trussing in, described (Col. Pasley)	iii.	222
Lary, near Plymouth,		
account of construction of (Mr. Rendel)	i.	99–108
use of wooden diving-bell in	„	103
caissons employed	„	105
composition of the mortar used in	„	106
Menai,		
undulations of, during gales	iii.	224
partially obviated by transverse trussing	„	*ib.*
account of injuries sustained by, in 1839 (Mr. Provis)	„	357–370
action of wind on, and oscillations	„	368
account of recent alterations in structure of (Mr. Maude)	„	371–376
Montrose,		
on injuries from hurricane in 1839 (Col. Pasley)	iii.	219
alterations proposed by Mr. Rendel	„	*ib.*
Staines,		
rise of arch, and opening in parapet, &c., due to changes of temperature (Mr. Rennie)	iii.	215
Southwark,		
rise of iron rib on changes of temperature (Mr. Rennie)	iii.	203–209
expansion of frieze in	„	213
Tewkesbury,		
account of construction of (Mr. Mackenzie)	ii.	1–14
wooden platforms for foundations	„	2
mortar of Aberthaw lime	„	3
Torpoint,		
account of floating bridge (Mr. Rendel)	ii.	213–227
nature of the site of bridge	„	214
drawbridge, and details of wood-work	„	216
description of engines and machinery	„	219
peculiar application of balance weights and chains	„	220
regulations and cost of bridge	„	224–227
Turin, over the Dora Riparia,		
account of construction of (M. Albano)	i.	183–194
method of ascertaining cut of voussoirs	„	187

	Vol.	Page
BRIDGES.		
TURIN, OVER THE DORA RIPARIA,—*continued.*		
method of laying the body of the arch, and cutting voussoirs	„	190
motion of voussoirs, and sinking on striking centres	„	192
YOUGHAL,		
account and construction of (Mr. Jones)	ii.	157–160
BRINDLEY, J.,		
brief account of, and works	i.	vi
BULL, W.,		
account of wooden bridge over the Calder	ii.	87, 88
BURY, EDWARD,		
on the locomotive engines of the London and Birmingham railway	iii.	305–342
CALORIC,		
action and force of, on boilers	iii.	42, 43
intensity of, distinct from temperature	„	44
power of, in peat coke, compared with coal	„	167
CANALS.		
Mr. Provis, on canal locks	i.	53–60
Mr. Field's improved canal lock	„	61
experiments on resistance to barges on	„	165–171
law of resistance deduced	„	172
Mr. Macneill's canal-boat experiments	„	237–282
dimensions of canals, boats and instruments described	„	238
confirmation of theoretical results		243
Mr. Green's perpendicular lifts for passing boats from one level to another	ii.	185–191
lifts applicable in cases of great height, short water, small boats, and downward trade	„	185, 190
CATARACT,		
use of, in regulating the rate of working engines	i.	124
use of, for opening the steam-valves suddenly	iii.	430
CENTRES,		
peculiar construction of, and method of striking, in bridge at Turin	i.	187, 197
in erecting Chester bridge	„	210
method of striking in Chester bridge	„	212
CHALLIS, PROFESSOR,		
theoretical conclusions of, confirmed by experiments	i.	243
CHAPMAN, WILLIAM,		
notice of, and works	i.	xvii
CHIMNEY,		
experiments on temperature at top of	ii.	167
CLARK, W. T.,		
account of the Gravesend pier (plates)	iii.	245–256
CLOTHING,		
various methods of clothing cylinders and boilers	i.	112
	ii.	53, 63
value of, in saving steam	ii.	163–172
	iii.	452
COAL,		
greatest efficiency of, not yet ascertained	ii.	161
results of different methods of using	„	162
Mr. Parkes' method of charging furnaces with	„	163
relative heating powers of, and coke	„	169
	iii.	34
tables of evaporative effect of	ii.	171–178
	iii.	45–48
tables of weight of, consumed in raising water, from the freezing to the boiling point	ii.	178, 179
strength of several kinds of, compared	„	19
various weights of bushels of	„	58
	iii.	68
value as a fuel in locomotives	„	33
the measure of, effect in engines	„	54
experiments with, in determining the constituent heat of steam	„	71
table of efficiency of, in the atmospheric, non-condensing, condensing, and Cornish engines	„	76
consumption of, per horse power in Lancashire and other engines	„	67, 76
See Coke, Combustion, Evaporation.		
COFFERDAM,		
use and construction of, for excavating rock at the bottom of rivers (Mr. D. Stevenson)	iii.	377–384
COKE,		
quantity produced from ovens and retorts at gas works	i.	109
produce of, from St. Etienne coal	ii.	169
relative heating powers of, and coal	„	176, 177
	iii.	34*n*
table of evaporation by, in locomotives	ii.	176
efficiency of, in locomotives	iii.	123
peat-coke, uses and calorific value of	„	165
Mr. Everett's analysis, &c. of peat-coke	„	166
possesses greatest intensity of heat, according to Berthier	„	177
COMBUSTION,		
theory of, to be studied in seeking evaporative economy in steam boilers	ii.	162
admission of air over fire to inflame the gases insufficient	„	*ib.*
Mr. Parkes' method of supplying air at the bridge of the furnace	„	164

	Vol.	Page
COMBUSTION,—*continued.*		
Mr. Parkes' method of charging the furnace, and the economy of unfrequent firing	ii.	165, 166
Davy's discoveries, and remarks on Mr. Parkes' process	„	162, 167*n*
importance of damper in regulating	„	164
	iii.	33
completeness and rate of, influence evaporative economy	„	4
rate of, least in Cornish boilers	„	8
influence of dimensions of grates and thickness of fuel, on	„	22
rates and practical effects of, in various boilers	„	9, 11, 25
rate of, in steam boilers, defined	„	36
table of rates of, and results in boilers	„	40
relative intensity of, in different boilers	„	42
intensity of, as affecting the durability and safety of the boilers	„	16, 17, 29 40, 42
intensity of, destructive of locomotive boilers	„	29
slow, conducive to economy of fuel	„	8
slow, conducive to durability and safety of boilers	„	44
of coke and coal, their relative effects	ii.	169
	iii.	33, 34
time during which products are in contact with boilers	iii.	35
of peat coke, compared with other fuels	„	167
COOPER, JAMES,		
account of restoring archstones of Blackfriars' Bridge	i.	131–2
CORNISH BOILERS,		
construction of, described (plate)	i.	118, 122
value of, compared with other boilers	iii.	9
consequences of slow combustion, extended fire-surface and thickness of plates, and proportions of parts of	iii.	9, 10
table of dimensions of parts of several	ii.	56
CORNISH ENGINE,		
Mr. Wickstead on the effective power of	i.	117–130
accuracy, and method of making monthly reports	„	120
reported duty not the whole performance	„	121
	ii.	62
method of working pumps of, described	i.	121
description of pump rods, balance bobs, valves and plungers, in	„	122, 123
applicable to water works	„	122
use of cataract in	„	124
	iii.	430
calculated pressure on expansion	i.	126
table of average duty of, in 1835 and 1836	ii.	68
Mr. Henwood on the expansive action of steam in some of	„	49, 60
duty performed in some, as shown by the indicator	„	53
CORNISH ENGINE,—*continued.*		
table of dimensions of various, and amount of load	ii.	54
table of water and steam, pressures and temperatures	„	55
table of dimensions of pumps in various	„	56
historical summary of improvements in	iii.	457
FOWEY CONSOLS,		
greatest performance on record	iii.	260
HOLMBUSH,		
dimensions of engine and pumps, and trial of	ii.	61
water raised at, weighed and measured	„	63
pressure of steam at different parts of stroke, estimated per engine at	„	64
HUEL TOWAN,		
water load at	iii.	262
dimensions of engine and pumps	ii.	54
water or steam consumed at, and other experiments on, by Mr. Henwood	„	53
doubts as to accuracy of reported duties removed on estimating duty by steam consumed instead of fuel	iii.	60, 258
consumption and economy of steam in, compared with other engines, and tabulated	„	76
Mr. Parkes, on the action of steam in	„	257
power and resistance of, examples of, analysed	„	259, 268
inefficiency of ordinary theories to account for effects of	„	268
economy of, in part attributable to percussive action of steam in	„	*ib.*
conditions under which it works examined	„	264, 270 417
various phenomena referable to steam's percussive action in	„	271, 279 417, 418
water load, the only resistance accurately ascertained	„	260
absolute resistance in, how compared	„	*ib.*
amount of, estimated in various cases	„	262
method of ascertaining the absolute and particular resistance to, shown	„	280
steam's elasticity at end of stroke compared with the resistance	„	263
value of steam's expansive and unexpansive action separated	„	*ib.*
economy of, independent of nominal expansion	„	274
action of cushion of steam in	„	280, 418
power of steam in, estimated by method of volumes	„	288
application of indicator to determine power of	„	288, 424
diagram of action of steam in	„	294
connexion between temperature of hot-well and pressure in the boilers	ii.	53
	iii.	273
retrograde motion at end of stroke explained	„	276
effect of mass of matter in motion by	„	264

	Vol.	Page
Crank Engine,		
condition under which it works compared with single acting engine	iii.	417
instantaneous action of steam on admission not available in	„	*ib.*
Cubitt, William,		
account of coals used at Ipswich gas-works during twelve months, and coke produced	i.	109
Curves.		
Mr. Bourns on setting out railway curves	iii.	295
may be performed with chain, table of off-sets and off-set staff	„	*ib.*
table of off-sets for each one-chain chord	„	303
Cushion		
of steam in Cornish engines, its use and action	iii.	280
influence of volume, on the duty	„	418, 421
of air, its resistance in explosion of gunpowder	„	421
Cylinder,		
various methods of clothing and jacketing in Cornish engines	i.	152
	ii.	53, 63
Damper,		
importance of use of, as a regulator of steam boiler fires	ii.	164
	iii.	33
Davy, Sir Humphrey,		
value of his discoveries on safety lamp and flame	ii.	162
remarks on Mr. Parkes' process of combustion under boilers	„	167
Denison, Lieutenant,		
experiments on American timber	ii.	15–32
Diving Bell,		
wooden, employed instead of cofferdam, at Lary Bridge	i.	103–5
Docks.		
See Hull.		
Donkin, Bryan,		
experiments on the discharge of water through different shaped orifices	i.	215–8
Donkin, John,		
account of borings for water in the vicinity of London	i.	155–6
Duty,		
origin and convenience of the term for commercial purposes	iii.	3
Duty,—*continued.*		
no change in use of term, or method of computing, since its introduction	iii.	449
various causes affecting	„	*ib.*
difference between actual and calculated	„	457
imperfect measure of whole performance of engines	i.	120
amount of, in various engines	ii.	53
table of duty of engines in Cornwall from 1769	„	67
table of average duty of, in 1835 and 1836	„	68
measure of useful effect in pumping engines	iii.	53
convertible into horse power	„	54
of the standard measure of	„	68
a given weight, 1 lb. of water as steam suggested as more correct standard than weight of coals	„	3, 69
table of, for various engines, measured by lb. of water as steam and bushel of coals	„	76
of locomotives. (*See* Locomotive Engine)	„	81
definition of, as applied to locomotives	„	55, 76
Mr. Enys on, at different periods, in Cornish engines	iii.	449–466
causes and summary history of improvements	„	456–9
of stamping engines	ii.	65
	iii.	463
Earth-work.		
Mr. Waterston on a method of estimating in excavations and embankments	i.	227–230
Economy,		
of fuel, secret of, in furnaces	ii.	163–7
of heat, the perfection of a boiler	iii.	49
of steam, the dynamic perfection of a steam engine	„	*ib.*
of evaporation, depends on rate and completeness of combustion and proportion of parts	„	4
of expansive working, defined as the increased quantity of action for given weight of water as steam	„	257
Effect,		
measure of, in pumping engines by duty, in rotary engines by horse power	„	53
useful, not a true measure of effective power	„	51
useful, defined	„	98
Efficiency		
of steam and fuel in various engines compared and tabulated	iii.	55, 76
Elastic Fluids.		
Mr. Webster on connexion between change of density and temperature	i.	219–225
practical consequences in working steam expansively	„	224
Engineering,		
early history and progress of	i.	i.–iv.

	Vol.	Page
Enys, John,		
on the duty of steam engines in the mines of Cornwall at different periods	iii.	449–466
Equilibrium Valve,		
application of, explained	ii.	50
Evaporation,		
experiments on, by Mr. Henwood	ii.	58
experiments on, by Mr. Parkes	„	161, 179
experiments on, by Mr. Loam	„	147
experiments on, by M. de Pambour	„	176
experiments on, by Mr. Watt	iii.	17
experiments on, by Mr. Smeaton	„	20
experiments on, by Mr. West	„	62
of different boilers at different periods	„	461
economy in, the secret of the economy of the steam engine	ii.	161
experiments on, with various coals, boilers, and methods of firing	„	*ib.*
effects on, of Mr. Parkes' method of firing and preventing smoke	„	162, 167
relative effects of coke and coal on	„	169, 177
	iii.	34*n*
importance of experiments on	ii.	170
tables of experiments on, with Cornish, waggon, and locomotive boilers	ii.	58, 171, 174, 176, 178
	iii.	45, 48
priming a source of inaccuracy in determining the amount of	ii.	176
	iii.	105, 106
in experiments on, temperature of feed water to be reduced to a common standard (Rule and Tables)	ii.	179
the measure of the useful effect of fuel in boilers	iii.	2
and duty combined, the measure of the useful effect of a given weight of water as steam in engines	„	*ib.*
the true test of different systems of constructing and firing steam boilers	„	*ib.*
Smeaton and Watt's attention to the subject of	„	*ib.*
progress in, since Smeaton's experiments on Newcomen's engine	iii.	20
hints to experimenters on	ii.	170
See Steam Boilers, Combustion.		
Expansion		
of water by heat (Mr. Tredgold)	i.	141
of various substances, table of	iii.	218
of steam, amount of, in Cornish engines	i.	126
of steam, principle of, published by Watt	„	127
of steam, principle of, applicable to rotary or double engines	ii.	64
of steam, economy of in engines, explained and defined	iii.	257
Expansion,—*continued.*		
of steam, value of, in what it consists	iii.	264
of steam, value of, how calculated	„	453
of steam, economy of, inversely as the steam's elasticity at the end of the stroke for given resistance	„	274
Expenditure of Power,		
on the, in engines	iii.	54
economy of, in different engines determinable only by weight of water consumed as steam	„	55
Explosion of Steam Boilers,		
some phenomena of, and suggested causes	iii.	425, 436
instances of bursting due to simple pressure rare	„	435
cannot be referred to the presence of hydrogen	„	437
may be owing to any sudden increase of pressure	„	438
experiments confirmatory of views	„	442
the bursting may be due to a succession of shocks	„	445
Exponents,		
term defined	iii.	9
Farey, John,		
on the temperature and elastic force of steam	i.	85–93
on the velocity of steam vessels through the water	„	111–6
Field, Joshua,		
on an improved canal lock	i.	61–6
experiments on the power of men	ii.	211–2
Fire-box,		
the proportions of, and effect of change of proportions	iii.	30–32
See Locomotives.		
Fire-grate,		
the proportions of, and effect of change in	iii.	30–32
See Grate.		
Floating Bridge.		
See Bridges.—Tor Point.		
Fluids.		
discharge of water through various orifices	i.	215
discharge of water through small pipes	ii.	201
action of aeriform	iii.	409
on connexion between change of density and temperature in elastic	i.	219–225
Fowey Consols Engine.		
See Cornish Engine.		
Fuel,		
weight or measure of, objections to as element in ascertaining the duty	iii.	49
See Coal, Coke, Peat.		

	Vol.	Page
FURNACE,		
of steam boilers, on proper management of	ii.	163–4
See Combustion, Grate, Steam Boiler.		
GAS,		
momentum of, when in motion in long pipes	iii.	432
GLYNN, JOSEPH,		
description of sawing machine for cutting railway bars (plate)	iii.	197–9
GRANITE,		
experiments on strength and durability of	i.	231–5
GRATES,		
proportion of, to fuel consumed affects completeness of combustion	iii.	4
table of rate of combustion on each square foot of, in various boilers	iii.	46
experiments with different proportions of	„	15, 22 *n.*
Tredgold's rules for, remarks on	„	23
See Combustion, Evaporation, Steam Boiler.		
GRAVATT, WILLIAM,		
account of sections through plastic clay in the neighbourhood of London	i.	151–3
GRAVESEND PIER,		
Mr. W. Tierney Clark's account of (plates)	iii.	245–256
history of pier and its consequences	„	245–9
formed on cast-iron piles	„	271
method of inserting piles by boring within a cast-iron shell	„	252
GREEN, JAMES,		
account of perpendicular lifts on the Grand Western Canal	ii.	185–190
GREENER, WILLIAM,		
his experiments on explosions of gunpowder cited	iii.	420
GRIFFITH, E.,		
his analysis of the Bog of Allen	iii.	165
designates Irish bogs as mines above ground	„	173
GRUNDY, JOHN,		
notice of his works	i.	x.
GUNPOWDER,		
experiments on, in barrel with open ends	iii.	420
action of, analogous to that of steam	„	432
GUY, HENRY,		
account of method of giving a true spherical figure to balls of metal, glass, &c., or other hard substance	ii.	47
HARDWICKE INDIAMAN,		
application of auxiliary steam to	iii.	398
log of voyage of	„	403–7
HARRISON, THOMAS E.,		
account of drops for shipment of coals at South Shields (plates)	ii.	69–70
HAYS, W. B.,		
account of a machine for cleaning and deepening small rivers (plate)	ii.	181–3
HEAT,		
expansion of water by	i.	141
experiments on at summit of chimney	ii.	167
loss of, from not clothing steam boilers	„	163, 172
time requisite for transmission of, to water in boilers	iii.	9, 28
velocity and duration of, about different boilers	„	37, 38
quantity and intensity of, about different boilers	„	41, 48
expenditure of truly measured, by the weight of water as steam consumed in engines	„	55
constituent heat of steam, experiments on	„	70
experiments on heat of steam in blast-pipe of locomotives	„	147
HENWOOD, W. J.,		
on the expansive action of steam in some of the Cornish pumping engines	ii.	49–60
HOLMBUSH ENGINE.		
See Cornish Engine.		
HORSE POWER		
of engines defined	iii.	54
convertible into duty	„	*ib.*
HOT BLAST,		
Mr. Neilson on use of, in the manufacture of iron	i.	81–83
HUDDART, CAPTAIN,		
notice of	i.	xviii.
HUEL TOWAN ENGINE.		
See Cornish Engine.		
HULL, KINGSTON UPON,		
Mr. Timperley's account of harbour and docks at, (plates)	i.	1–52
general history of, and tides at	„	1–3
Old dock and account of works at	„	3–14
Humber dock and account of works at	„	14–29
Junction dock and account of works at	„	29–50
table of dimensions of docks, basins, locks, bridges, warehouses, sheds, and quays	„	51–52

	Vol.	Page
IMPACT,		
action of steam on piston in Cornish engine on its admission, likened to	iii.	268
on the air in the case of explosion of gunpowder in barrel with open ends	„	421
of steam in Perkins' gun	„	429, 433
of elastic fluids on solids	„	430
INDICATOR,		
construction and application of, described	ii.	49
experiments with, on Cornish engines	„	50–1
curve described and analysed	„	*ib.*
invented by Watt, its object	iii.	51–2
its indications of power must be compared with the force to be deduced from the method of volumes	„	52
on the, as applied to Cornish engines	„	283
diagrams from Cornish engines illustrative of the percussive action of steam	„	271
diagrams from rotative engines illustrative of the percussive action of steam	„	410, 427
precautions to be taken in the use of	„	422, 433
requisite care in experiments with	„	455
INSTITUTION OF CIVIL ENGINEERS,		
account of origin and progress of	i.	xxi.
IRON,		
manufacture of, by hot blast	i.	81–83
principle of preceding, and saving effected by	„	*ib.*
cast, effects of water on	i.	203
series of experiments on the strength of (Mr. F. Bramah)	ii.	113–135
expansion of, experiments on	iii.	212–5
strength of, how affected by cold	„	212
extension of, before breaking	„	440
effect of sudden application of weights on	„	441
IRON PILING,		
history of, and use at Brunswick Wharf, Blackwall	i.	195–205
JACKET,		
effect of, on cylinders and boilers	ii.	53
correction for amount of effect of	iii.	452
general uses and advantages of	„	*ib.*
JESSOP, WILLIAM,		
notice of, and his works	i.	x.
JONES, J. E.,		
account and description of Youghal Bridge	ii.	157–160
LARDNER, DIONYSIUS,		
experiments on locomotives examined	iii.	109–121
LIFTS,		
Mr. Green's construction of, for passing boats from one level to another on canals	ii.	185–192
peculiarly advantageous for small boats, downward trade and short water	„	*ib.*
LIGHTHOUSES,		
Captain Smith on method of illuminating	ii.	193–100
distinct principles of fixed and revolving lights	„	194
mechanical arrangements for reciprocating light	„	197
LIVERPOOL,		
steam ship, advantage of diminished power in	iii.	387
LOCKS,		
Mr. Provis on locks used for canal and river navigation	i.	53–60
with double gates, and with or without chamber walls	„	55–6
dimensions of, on Caledonian Canal	„	56
circular lock on Languedoc Canal	„	*ib.*
double, and chain of, connected longitudinally	„	59
Mr. Field on an improved lock	„	60–5
See Canals.		
LOCK GATES,		
construction of, at Hull Docks	i.	17
Mr. J. W. Barlow on strain of	„	67–80
curved form and angle of sally of	„	67
nature of pressures and strains on, expressed in terms of angle of sally	„	70
angle and form of greatest strength	„	72, 74
table of transverse strains at given angle and different depths	„	72
angles of, in this country, at Muyden, and Sparendam	„	77
dimensions of, at St. Katherine's, London, West India, and Dundee Docks; Sheerness Basin and Caledonian Canal	„	78–80
LOCOMOTIVE BOILER.		
See Steam Boiler.		
LOCOMOTIVE ENGINES,		
Mr. E. Woods on certain forms of	ii.	137–155
causes and progress of improvements in	„	137–8
on four and six-wheeled, inside and outside framing	„	139–141
objections to six wheels considered	„	142
distribution of weight on the wheels	„	144
crank axles and outside crank pins	„	145
relation of forces and side pressures in	„	147–9
coupled and uncoupled engines	„	151
Mr. Melling's friction or locking apparatus	„	182
Mr. Josiah Parkes on	iii.	77–162
limit to power of	„	33
differs only in the blast from the fixed non-condensing engines	„	77, 89
the work done by, and duty of	„	81, 96
duty and horse power calculated	„	85
gross and useful effect of, distinguished	„	82
effective horse power of, defined	„	*ib.*
power of, determined by the relative volumes of water and steam consumed	„	83
horse power of, compared with weight of water consumed as steam and coke	„	85

	Vol.	Page
LOCOMOTIVE ENGINES,—*continued.*		
8 lbs. per ton as resistance to progressive motion incorrect	iii.	89
effect of, measured by momentum of	„	124
effective power of, expended solely in generating momentum	„	*ib.*
nature of various resistances to	„	125
useful performance of, measured by the momentum generated by the equal expenditure of water as steam in equal times	„	126
momentum of, and duty, expressions of practical effect	„	140
resistance in, due to the blast	„	143
expenditure of power by, and fixed non-condensing engines compared	„	153
experiments on, by M. de Pambour, examined	„	78, 102
experiments on, by Mr. R. Stephenson, examined	„	102
experiments on, by Mr. N. Wood, examined	„	107
experiments on, by Dr. Lardner, examined	„	*ib.*
Mr. Barlow on power of, at different velocities	„	183–196
power expended by, may be ascertained	„	183
resistances at different velocities, and causes of	„	187, 190
mean speed on planes of different inclinations	„	194
Mr. E. Bury on locomotive engines of London and Birmingham Railway	„	305–342
average fuel and repairs per mile per ton of goods	„	306
parts of four-wheeled engines described (plates)	„	307–310
inside and outside framings compared	„	311–2
comparative advantages of four and six wheels	„	313–4
tables of miles run, coke, oil, &c., and repairs for six months	„	318–342
See Steam Boiler, Combustion, &c.		
LONG BENTON ENGINE.		
The gauge of Smeaton's atmospheric engine	iii.	3
particulars of, and boilers; their effect (Tables)	„	45, 76
See Atmospheric Engine, Smeaton, &c.		
MACKENZIE, WILLIAM,		
account of Tewkesbury Bridge	ii.	1–14
MACNEILL, JOHN,		
account of canal-boat experiments	i.	237–282
MATERIALS,		
theoretical rule for thickness of, in hydraulic press	i.	133
strength of American timber	ii.	15–32
strength of cast iron	„	113–134
effect of temperature on strength of	iii.	201–218
MERCURIAL GAUGE,		
use of, for determining resistance to engines	iii.	280
experiments with, illustrative of instantaneous action of steam and aeriform fluids	„	411, 432
phenomena of oscillation of, explained	„	419
MOMENTUM,		
a measure of effective power of locomotive engines	iii.	126, 140
origin, destruction, and cause of, in Cornish engines	„	264–5
of steam, distinguished from simple pressure	„	268
of steam, value of, approximately ascertained	„	412, 415
of gas in main	„	432
MORTAR,		
some experiments with various kinds of	i.	45–6
composed of blue lias, puzzolana, and sand	„	106
composed of Aberthaw lime	ii.	8
MYLNE, R. W.,		
on the supply of water from Artesian wells in the London Basin, and account of well sunk at Hampstead Road Reservoir	iii.	229–244
NEILSON, JOHN B.,		
on the hot blast in the manufacture of iron	i.	81–3
NEWCOMEN,		
his character as an inventor	iii.	58–9
OUTRAM, FRANCIS,		
description of method of roofing in use in the Southern Concan	i.	157–164
PALMER, G. H.,		
on the application of steam as a moving power in reference to the economy of high and low steam	ii.	33–46
PALMER, H. R.,		
experiments on resistance to barges moving on canals	i.	165 173
PAMBOUR, CHEVALIER DE,		
experiments of, on heating surface examined	iii.	80
results of theory of, on locomotive examined	„	78–92
results and conclusion of, in new theory of steam engines	„	93–102
PARKES, JOSIAH,		
on the evaporation of water from steam boilers	ii.	160
on steam boilers and steam engines	iii.	1–162
on the action of steam in Cornish pumping engines	„	257–294
on the percussive or instantaneous action of steam, and other aeriform fluids	„	409
PASLEY, COLONEL,		
on the Montrose bridge after the hurricane, and on the effect of wind on suspension bridges generally	iii.	219–227
PEAT,		
analysis of, and peat coke	iii.	166
various uses to which applicable	„	173
See Turf.		

Vol. Page

PELLATT, APSLEY,
experiments on the relative heating power of coal and coke . . . . . iii. 34

PERCUSSION
of steam and other elastic fluids . . . . . iii. 257, 409

PILE DRIVING,
general observations on . . . . . i. 32–34

PILES,
iron, history and use of (Mr. Borthwick) . . . i. 195–203
iron, use of, and method of driving through chalk, in foundations of Gravesend pier . . . . . iii. 251
See various works, Hull Docks, Bridges, &c.

PLUNGERS,
solid, used in Cornish engines . . . . . i. 123
history of introduction of . . . . . iii. 462

POWER,
effective, how measured in steam engines . . . iii. 51
absolute, shown by indicator and method of volumes „ 52
expenditure of, truly measured by weight of water as steam . . . . . „ 55
apparent deficiency of, in Cornish engines . . . „ 266
of given quantity of steam, two methods of measuring . . . . . „ 450
when measured by boiler supply . . . . . „ 451
of men, Mr. Field's experiments on . . . . ii. 211–2
adaptation of steam power to vessels . . . . iii. 386
advantages of diminished, in vessels . . . . . „ 387

PRESS,
Hydraulic or Bramah, rule for thickness of cylinders in . . . . . i. 133

PRIMING,
a source of great inaccuracy in experiments on evaporation . . . . . ii. 176
iii. 105, 106

PROVIS, W. A.,
on locks used for canal and river navigation . . i. 53–60
experiments on the flow of water through small pipes ii. 201–210
on the effects of wind on the Menai bridge and the damage sustained in January, 1839 . . . . iii. 357–370

PUMP RODS
in Cornish engines described . . . . . i. 122

PUMPS,
description of those used in several Cornish engines i. 123
plunger pole, author of introduction of . . . . iii. 462

Vol. Page

RAILS,
Mr. Glynn's description of sawing machine for cutting . . . . . iii.

RAILWAYS,
Mr. Reynolds on construction of, with continuous bearing . . . . . ii. 76–86
conditions essential to perfection of . . . . . „ 73–4
methods of ensuring requisite conditions and lateral and vertical stability of rails . . . . . „ 76–84
description of rail of continuous bearing for, (plate) „ 85–6
on setting out curves for . . . . . iii. 295–303

REDMAN, JOHN BALDRY,
account of the construction of Bow bridge . . iii. 343–356

RENDEL, JAMES,
account of the construction of the Lary bridge . i. 99–108
account of the construction of the floating bridge across the Hamoaze at the Tor Point . . . . ii. 213–227

RENNIE, GEORGE,
on the expansion of arches . . . . . iii. 201–218

RENNIE, JOHN,
notice of, and his works . . . . . i. xii.

REPORTS
of steam engines in Cornwall, method of making . i. 120

RESISTANCES,
Mr. H. R. Palmer on, to barges on canals . . . i. 165–17.
methods used, and causes of imperfect results in . „ 166
law of, nearly as the cube of the velocity . . . „ 172
table of ratio of power to load at different velocities „ 173
Mr. Macneill's experiments on, in canals . . . i. 237–244
rise of boat during motion predicted by theory . „ 243
In engines.—*See* Cornish, Locomotives.

REYNOLDS, JOHN,
on the construction of railways of continuous bearing . . . . . ii. 78–86

RIVERS,
Mr. Hays on a machine for scouring small . . . ii. 181–183

ROOFING,
description of, in Southern Concan, with stone . . i. 157–163

ROBINS, BENJAMIN,
experiments on gunpowder . . . . . iii. 432

SAILING VESSELS,
on the adaptation of steam power to (Mr. S. Seaward) . . . . . iii. 385–408

	Vol.	Page
SAWING MACHINE		
for railway bars, description of, (plate) (Mr. Glynn)	iii.	197–199
SCHAFHAEUTL, DR. CHARLES,		
on the circumstances under which explosions frequently occur in steam boilers, and the causes to which they may be assigned	iii.	435–448
SEAWARD, JOHN,		
on procuring supplies of water for towns by boring	i.	145–150
SEAWARD, SAMUEL,		
memoir on the practicability of shortening the duration of voyages by the adaptation of auxiliary steam power to sailing vessels	iii.	385–408
SIMPSON, JAMES,		
report of, on well in Hampstead-road	iii.	241–244
SMEATON, JOHN,		
notice of, and works	i.	viii
his practice as to evaporation, and experiments on fuel	iii.	20, 24
SMOKE,		
methods of preventing, by Watt and Parkes	ii.	162, 164
See Combustion.		
SOUTHERN, MR.,		
experiments on temperature and elastic force of steam	i.	86
SPHERES,		
method of making perfect, of hard substance	ii.	47
STAMPING ENGINE,		
experiment on duty of	ii.	66
	iii.	463
STEAM,		
Mr. Farey, on relation between temperature and elastic force of, when in contact with water	i.	85–93
Southern's experiments on, prior to those of French Academy	„	85
tables of elastic force and temperature of	„	90
change in temperature on sudden expansion of	„	222
consequences of preceding in practice of steam engine	„	224
Mr. G. H. Palmer, on application of, as a moving power	ii.	33–46
quantity of water raised by a cubic foot of steam in vacuo	„	35
consequences of law, that the sum of latent and sensible heat is constant	„	36, 37
quantity of water in, diminishes with temperature	„	40
causes of loss of, in working at full stroke	„	45
correction for deficiency of water in high	iii.	451
STEAM,		
pressure of, in cylinder, methods of ascertaining	ii.	50
	iii.	454
generation and application of, distinct problems	„	49
methods of determining power of, in engines	„	51, 53
experiments on the constituent heat of	„	70
consumption of, in different classes of engines	„	58
table of consumption of, in different class of engines	„	76
tables of consumption of, by locomotive engines	„	123
volumes of, and water compared by M. de Pambour	„	122
resistance of, from blast in locomotives	„	143
expenditure of, by fixed and locomotive non-condensing engines	„	153
the action of, in Cornish engines (Mr. Parkes)	„	257
economy of, defined	„	*ib.*
Cornish system of using	„	258, 270
data for investigating action of, in engine	„	259
analysis of the action of, in three Cornish engines	„	260
elasticity of, at end of working stroke, and importance of determining	„	257, 263
expansive and unexpansive, value of	„	263
absolute power of, compared with absolute resistance in Cornish engines	„	268
diagram of action of, in Cornish engines	„	294, 409
percussive action of, stated and defined	„	268, 409
percussive action of, method of measuring	„	269, 422
percussive action of, approximate value of	„	415
percussive action of, shown by indicator	„	271, 411
percussive action of, shown by mercurial gauge	„	411
percussive action of, shown by Perkins' gun	„	412, 429
percussive action of, shown by springing of cylinder covers	„	417
percussive action of, shown by various phenomena illustrative of	„	272, 276
		417, 418
		420, 429
percussive action of, only exhibited under certain conditions and methods of working	„	416, 417
application of, as auxiliary to sailing vessels	„	385–508
causes of limited application of, to long voyages	„	386
power, ratio of, to dimensions of vessels	„	388
power alone unfit for very long voyages	„	390
deficiency of water in high pressure steam	„	451
STEAM BOILERS,		
construction of Cornish, described (plate)	i.	118, 122
dimensions of several Cornish	ii.	55, 56
loss of heat in, from not clothing them	„	172
Mr. Parkes on	iii.	1, 48
various classes of, analysed	„	5
relations of several parts to the effects of	„	7
influence of time of duration of heat about	„	8, 35
effects of structure, and mode of setting, on duration of heat about	„	35
slow combustion essential to economy, safety, and durability of	„	8, 44

	Vol.	Page
STEAM BOILERS,—*continued.*		
economy of heat in some, proportionate to extent of heated surface in	iii.	8, 44
economy of heat constitutes the evaporative perfection of	„	49
arrangement, practice, and effect of Cornish	„	9
arrangement, practice, and effect of waggon	„	11
arrangement, practice, and effect of locomotive	„	25
tables of dimensions, and results of numerous experiments on	ii.	171, 174 176, 178
	iii.	45, 48
rules as regards proportions of parts, grates, and practice in working	„	21
Tredgold's rules for grates of, examined	„	23
proportions of, to various engines	ii.	166
	iii.	56
priming of, a defect in	ii.	176
	iii.	105, 106
supply of steam from, a measure of power	„	451
See Cornish, Waggon, Locomotive, Combustion, Smoke, Evaporation.		
STEAM ENGINES,		
Mr. Parkes on	iii.	49
economy of steam, the dynamic perfection of	„	*ib.*
proportions of various boilers to	ii.	166
	iii.	56
progress of improvements in	„	58, 61
methods employed to determine the power of	„	51
measures of effect of, defined	„	53
duty and horse power of, explained	„	*ib.*
weight of water as steam, best practical test of expenditure of power	„	54
table of relative economy of steam and fuel in various	„	76
velocity of working stroke in	ii.	58
	iii.	272
See Atmospheric, Cornish, Locomotive, Steam.		
STEAM GUN,		
experiments with, cited as illustrative of instantaneous action of steam on piston	iii.	429
STEAM VESSELS,		
Mr. Farey, on velocity of, through the water	i.	112–116
limited application of, for long voyages	iii.	385
various causes of preceding limits	„	386
effects of diminished power and weight on speed of	„	387–389
STEVENSON, DAVID,		
description of a cofferdam adapted to a hard bottom, and used in excavating rock from the Ribble	iii.	377–384
STONES,		
experiments on strength of various	i.	235
STONES,—*continued.*		
wear of, on tramway on Commercial-road	i.	233
Mr. Rennie's experiments on expansion of	iii.	215
M. Destigny's experiments on expansion of	„	217
Mr. Adie's experiments on expansion of	„	202
SYLVESTER, CHARLES,		
analysis of consumption of steam by condensing and non-condensing engines	iii.	154
TAYLOR, JOHN,		
chronological table of duty of engines in Cornwall	ii.	67
TELFORD, THOMAS,		
notice of, and works	i.	xiv
TENSION		
of bodies, or resistance in the direction of their length	i.	175
not always proportional to area of section	„	*ib.*
conditions under which preceding obtains	„	176
effect of change in direction of straining force	„	179
comparison between pressive and tensile forces	„	180
TIMBER,		
strength of, loaded at the end, Girard's experiments	i.	69
strength of American, Lieutenant Denison's experiments	ii.	15–32
TIME,		
influence of, in respect of duration of heat about boilers	iii.	8, 35
general consideration respecting, in connection with boilers	„	36–39
See Combustion, Steam Boilers.		
TOWNS,		
supply of, with water by boring (Mr. Seaward)	i.	145–150
TREDGOLD, THOMAS,		
on the expansion of water by heat	i.	141–144
on the principles of tension, and resistance of bodies to being torn asunder in the direction of their length	„	175–181
his rules for grates examined	iii.	23
TRUBSHAW, JAMES,		
account of construction of Chester-bridge	i.	209–214
TUNNELS,		
on ventilation and lighting of	i.	95–98
TURF		
employed in working some of the Cornish engines	i.	117

	Vol.	Page
Turf and Turf Coke,		
Mr. Williams, on the preparation, properties, and uses of turf and turf coke	iii,	162–181
value of, in the working of iron	„	164
upper portion of bog most valuable in respect of heating qualities	„	165
analysis of	„	166–169
value of litharge test of Berthier for determining the calorific powers of coke	„	169, 170
process of manufacture of turf into coke	„	171
peculiar use of, when manufactured	„	173–178
value of, as a fuel, when combined with resin	„	179
value of, tested in voyages across Atlantic	„	*ib.*
Unit		
of heating surface, experiments for ascertaining	iii.	30, 31
United Mines,		
experiments on engine at (Table)	iii.	61
Useful Effect		
not synonymous with effective power	iii.	51
Valves,		
dimensions of, in various Cornish engines	i.	123
	ii.	54
relative area of steam, and cylinders in Cornish engines	iii.	272
difference in effect of opening, suddenly and slowly	„	411–417
double beat skeleton	„	430
Velocity		
of steam entering cylinder, approximation to	iii.	272
of working stroke in Huel Towan engine	„	*ib.*
of both strokes in several Cornish engines	ii.	58
of beat about boilers, comparative rates of	iii.	37
of blast steam in locomotives	„	152
of water in Cornish lifts	„	261
Vernon,		
auxiliary steam power adapted to the	iii.	393
description of the engines and machinery of	„	393
results of outward and return voyage of	„	396, 397
second voyage of, compared with that of the India	„	399
Volumes,		
method of, for determining power of engines	iii.	52
table of, at different temperatures	„	122
steam pressure at end and different parts of stroke determined by	„	263, 270
examples of computation of power by	„	288
Voussoir,		
peculiar method of cutting, in bridge of Nemours	i.	189
in the bridge at Turin	„	190
Waggon Boilers,		
rates of combustion and evaporation in	iii.	12
compared with Cornish boilers	„	*ib.*
various experiments and observations on	„	13–24
See Steam Boilers.		
Walker, James,		
on the ventilation and lighting of tunnels	i.	95–99
experiments on the wear of stones on the tramway on the Commercial-road	„	233, 234
Walker, Ralph,		
notice of, and works	i.	xvii.
Walls,		
openings in, on changes of temperature	iii.	202
Water,		
expansion of, by heat, experiments on	i.	141
Mr. Seaward, on supply of towns with, by boring	„	145–150
supply of, by boring, limited and precarious	„	146–147
supply of, from chalk formation and sand of London basin	iii.	242
borings for, in neighbourhood of London	i.	155, 156
effects of, on cast iron	„	203, 204
discharge of, through different shaped orifices	„	215–218
Mr. Provis on flow of, through small pipes	ii.	201–210
tables of experiments and results	„	203–209
raised by Cornish engines, weighed and measured	„	61, 62, 58*n*
	iii.	4
difference between actual and calculated quantity raised	„	451
quantity of, consumed as steam in engines, ascertained by Smeaton and Watt	„	2, 3
weight of, as steam, best practical measure of dynamic efficiency and engine economy	„	2, 55
pound of, as steam, suggested as standard of performance of engines	„	68, 69
table of consumption of, by various engines	„	76
temperature of, in hot well inversely as pressure of steam in boiler	ii.	55
	iii.	273
load in Cornish engines, the only ascertained resistance	„	261
resistance of, in pumps	„	261*n*
See Evaporation.		
Water Works,		
applicability and advantage of Cornish engines for	i.	122–124
Waterston, John James,		
on a method of representing by diagram the earthwork in excavations and embankments	i.	227–230

	Vol.	Page
Watt, James,		
his experiments on the consumption of steam and coal at the Albion Mills engines	iii.	3, 17, 45, 76
character of, as an inventor	„	24, 58-59
his experiments on steam the basis of his discoveries	„	3
effects of his practice compared with present Cornish	„	56
the perfector of the unexpansive engine	„	3, 59
Webster, Thomas,		
on the changes of temperature consequent on a change in the density of elastic fluids, particularly steam	i.	219–225
Wells,		
Artesian, on supply of water from (R. W. Mylne)	iii.	229–233
particulars of various, in London basin	„	230–233
account of well at Hampstead-road reservoir	„	234–240
peculiar difficulties of, from sand springs	„	*ib.*
application of successive sets of cylinders in sinking	„	235
sinking of, with water in the well	„	237
quantity of water raised from well in Hampstead-road	„	239
Wheels,		
Professor Willis, on the teeth of	ii.	89–112
the various curves adapted for	„	89
involute and evolute teeth	„	90
method whereby all wheels of same pitch may work together	„	91–93
a form of increased strength for, suggested	„	93, 94
approximation to true theoretical form by arcs of circles	„	95–100
Wheels,—*continued.*		
teeth of, described, consisting of single and double arcs	ii.	101-109
teeth of, working with trundles or radial flanks	„	110
cutters for	„	111, 112
Wicksteed, Thomas,		
on the effective power of the high pressure condensing Cornish engine	i.	117–130
	ii.	61–68
Williams, Charles Wye,		
on the preparation, properties, and uses of turf and turf coke	iii.	162–181
Willis, Professor,		
on the teeth of wheels	ii.	89–112
Wind,		
effect of, on suspension bridges	iii.	220
effect of, on a roof of a shed at Chatham	„	221
effect of, on chain pier at Brighton	„	222
suggested means of obviating effects of, on suspension bridges	„	366 368
Wood, Nicholas,		
experiments of, on Great Western railway	iii.	107
Woods, E.,		
on certain forms of locomotive engines	ii.	137 155

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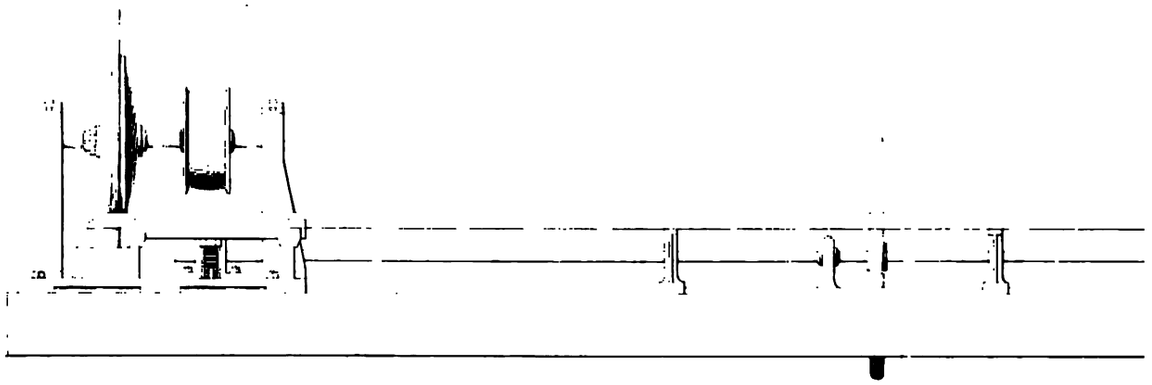
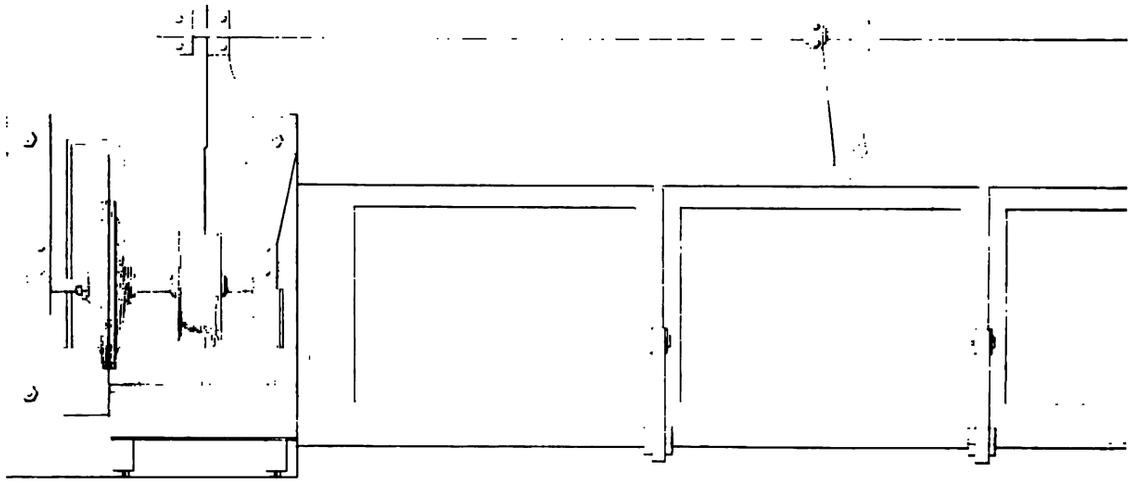


ELEVATION. FIG. 1.



PLAN. FIG. 2

FIG. 4

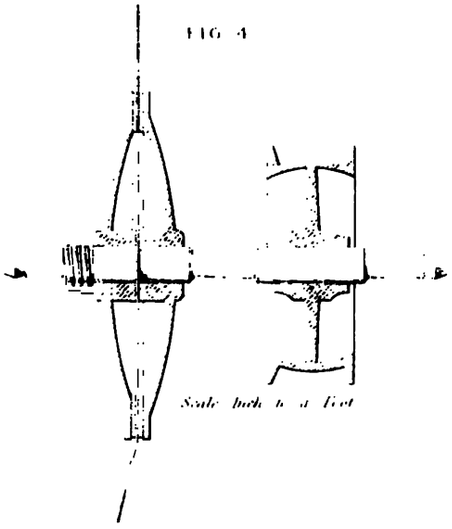
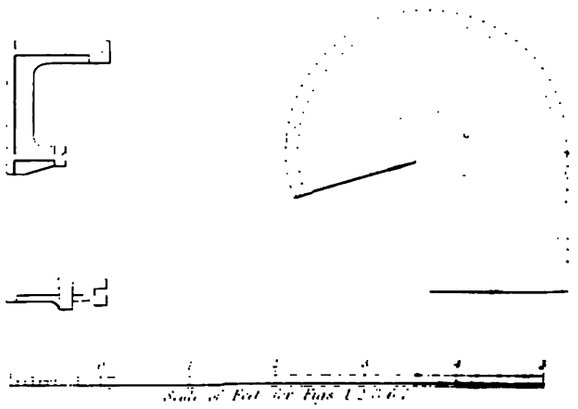
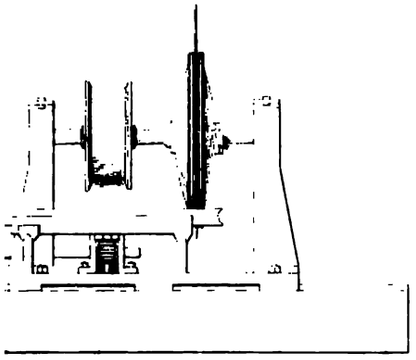
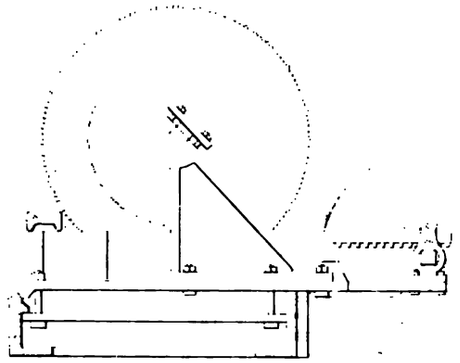


FIG. 6



TING RAILWAY BARS.





END VIEW. FIG. 3.

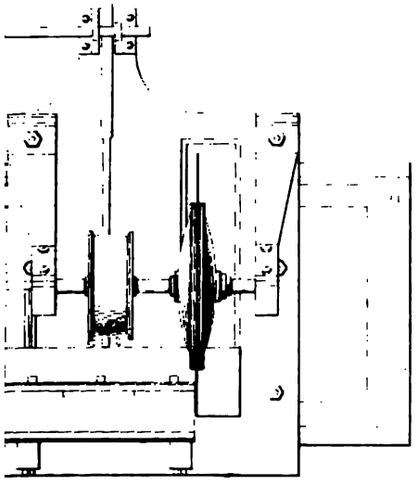


FIG 7.

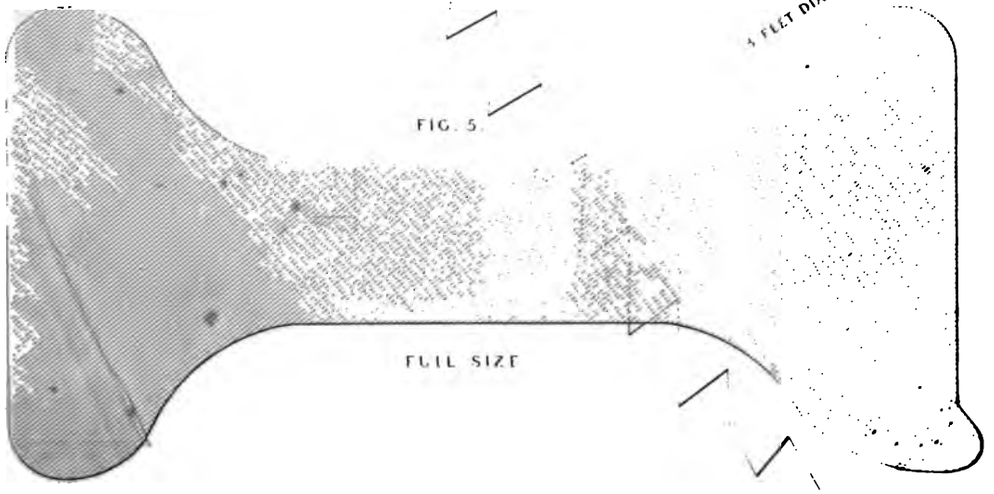
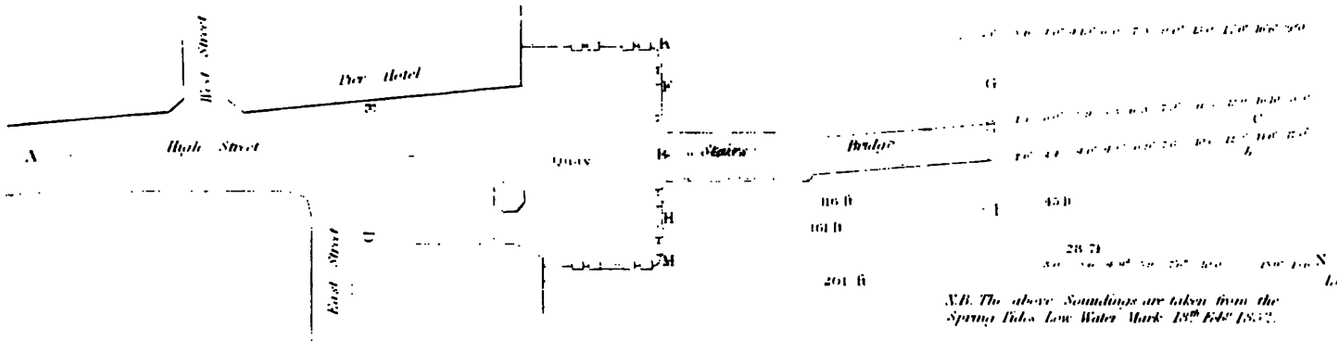


FIG. 5.

GRAVESEND PIER.

W. TIERNEY CLARK



N.B. The above Soundings are taken from the Spring Tides Low Water Mark 18th Feby 1832.

RISE OF TIDE AS TAKEN AT GRAVESEND PIER.

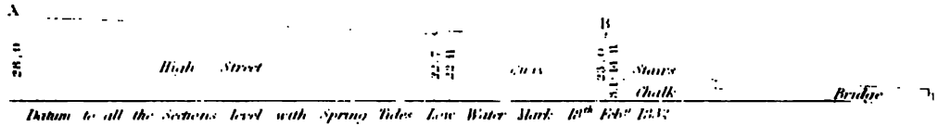
18th February 1832.

High Water Mark 20.0 / 20.3 — *17th February. 18 / 19*

18.0	2	*O'Clock*	17 *Minutes*
17.9	2		1
16.8	1		48
15.8	1		33
14.8	1		18
13.7	1		2
12.0	12		37
10.5	12		12
8.10	11		50
7.4	11		30
5.6	11		10
4.2	10		58
2.6	10		30

Low Water Mark 18 Feby 1832.

SECTION A. B. C.



Datum to all the Sections level with Spring Tides Low Water Mark 18th Feby 1832

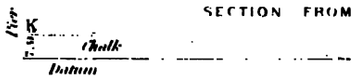
RISE OF TIDE AS TAKEN OUTSIDE THE ENTRANCE LOCK TO THE THAMES AND MEDWAY CANAL.

17th February 1832.

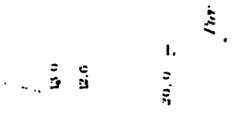
Wall Top at Quoin 25.9 / *Level with* 25.0 — *West Side of Lock Gate Gravesend Pier*

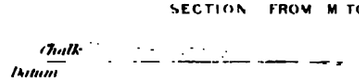
20.9	2	*O'Clock*	0 *Minutes*
20.0	1		52
19.0	1		15
18.0	1		1
17.0	12		19
16.0	12		34
15.0	12		19
14.0	12		4
13.0	11		53
12.0	11		37
11.0	11		17
10.0	11		3
9.0	10		50
8.0	10		40
7.0	10		30
6.0	10		15

New Sill, entrance 1.6 to canal Lock.
Old Sill, at 18 1 Inch above Low Water Mark 17th Feby 1832

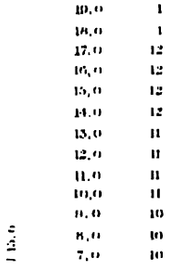


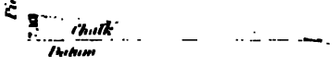
SECTION FROM K TO L





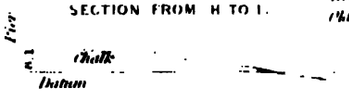
SECTION FROM M TO N.



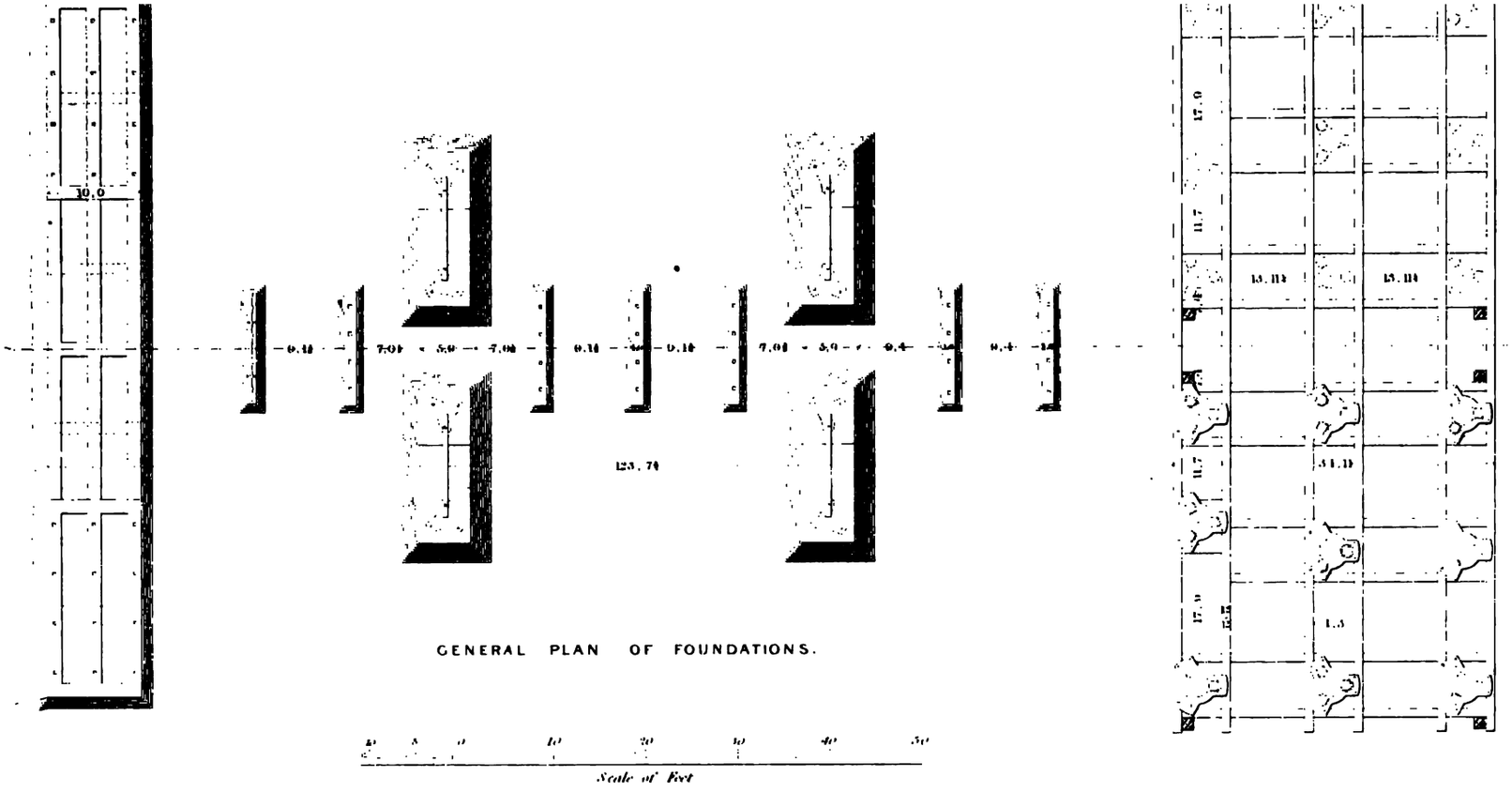


SECTION FROM F TO G.

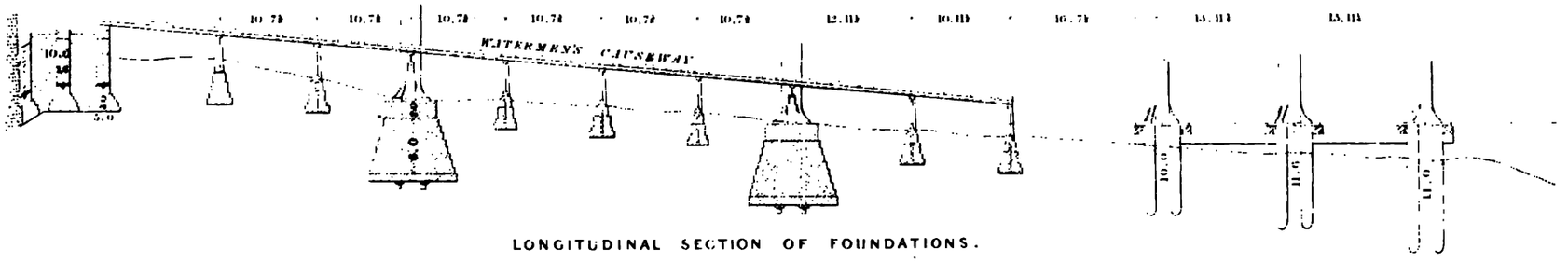
SECTION FROM E TO D.



SECTION FROM H TO I.



GENERAL PLAN OF FOUNDATIONS.



LONGITUDINAL SECTION OF FOUNDATIONS.

Original drawings by Fred.k Rumble.

J.W. Lowry Sculp

Institution of Civil Engineers, 1846

GRAVESEND PIER.

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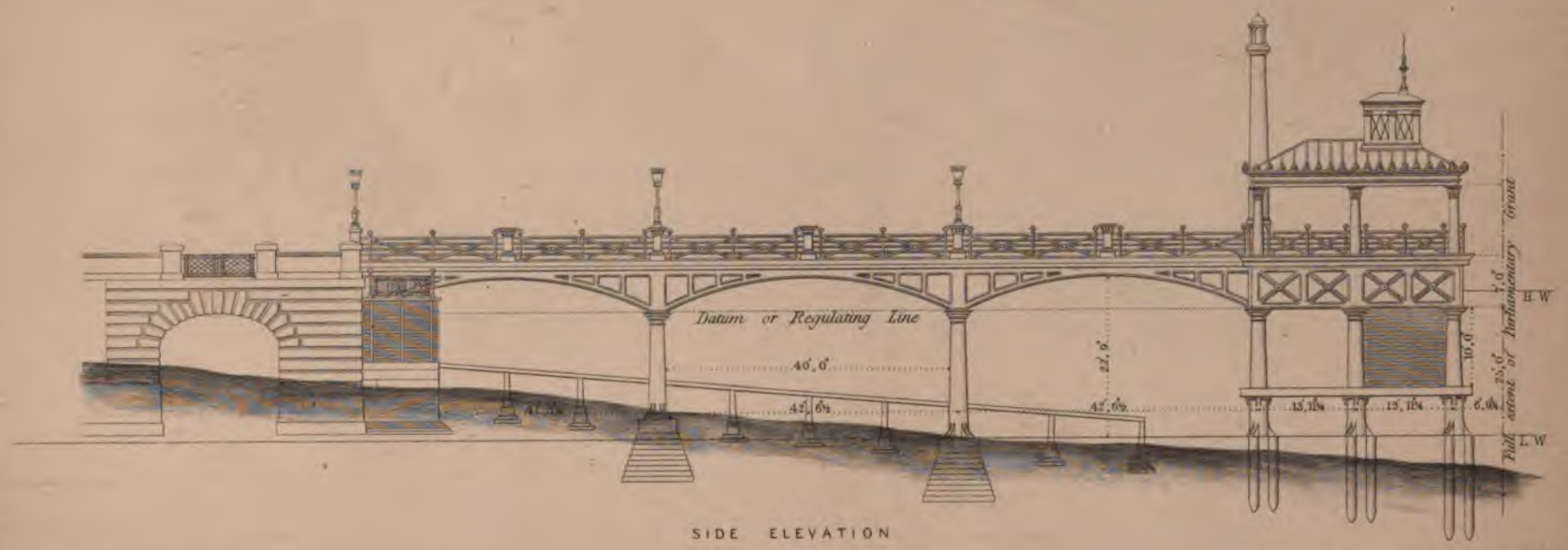
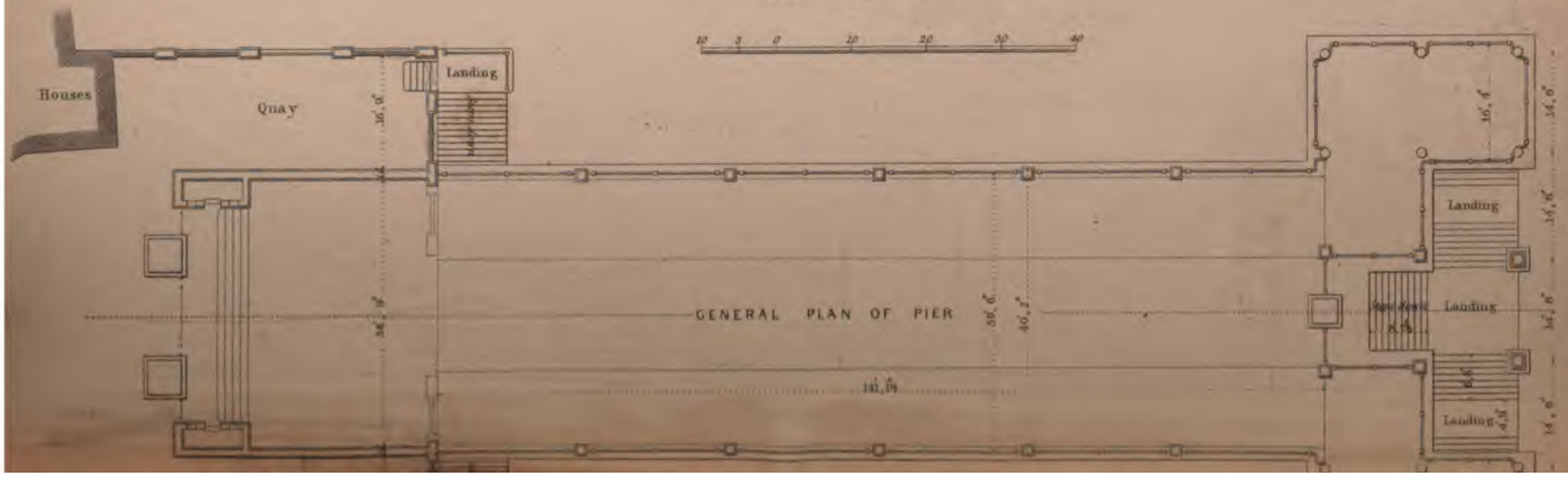
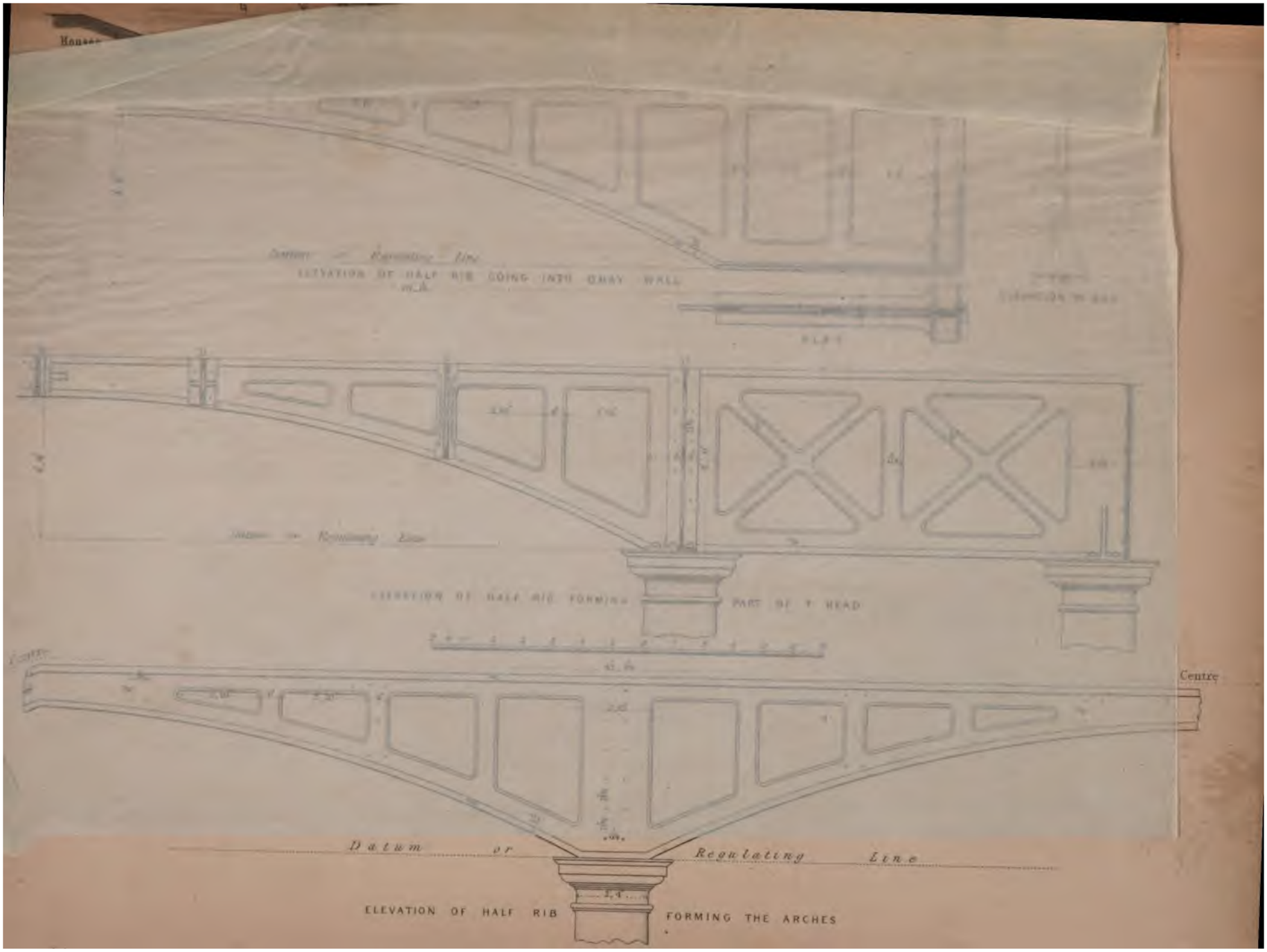


SIDE ELEVATION

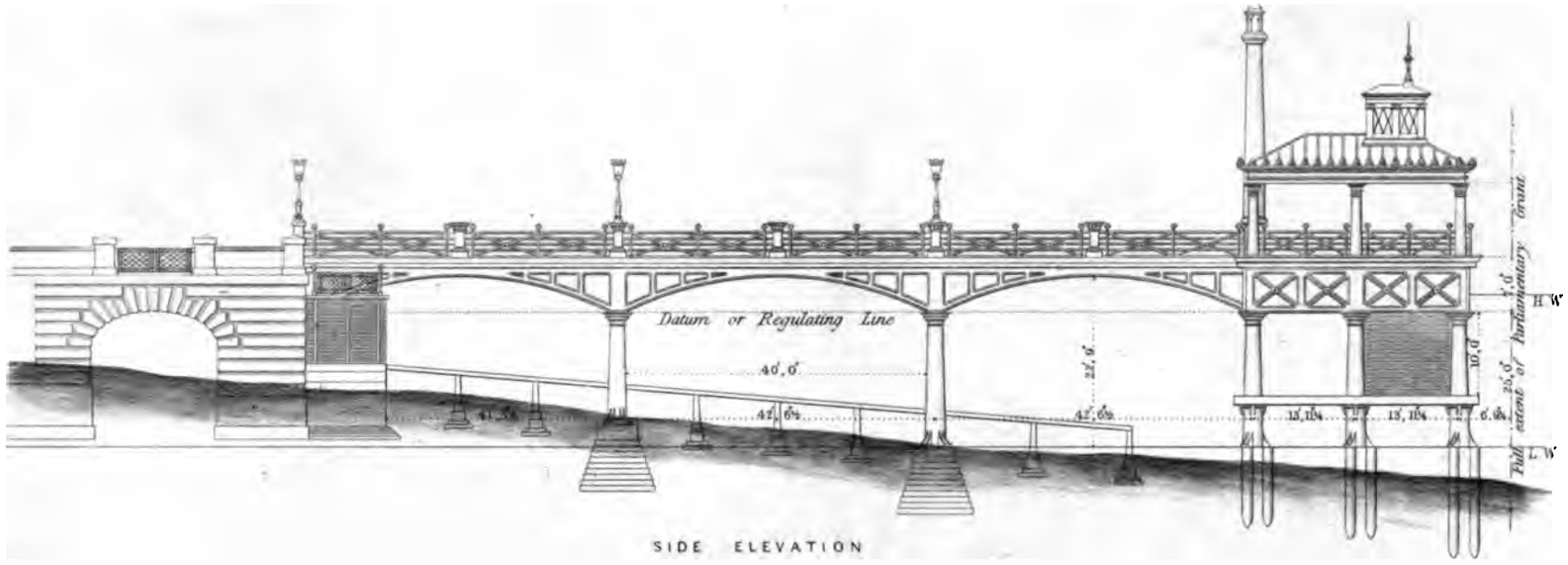


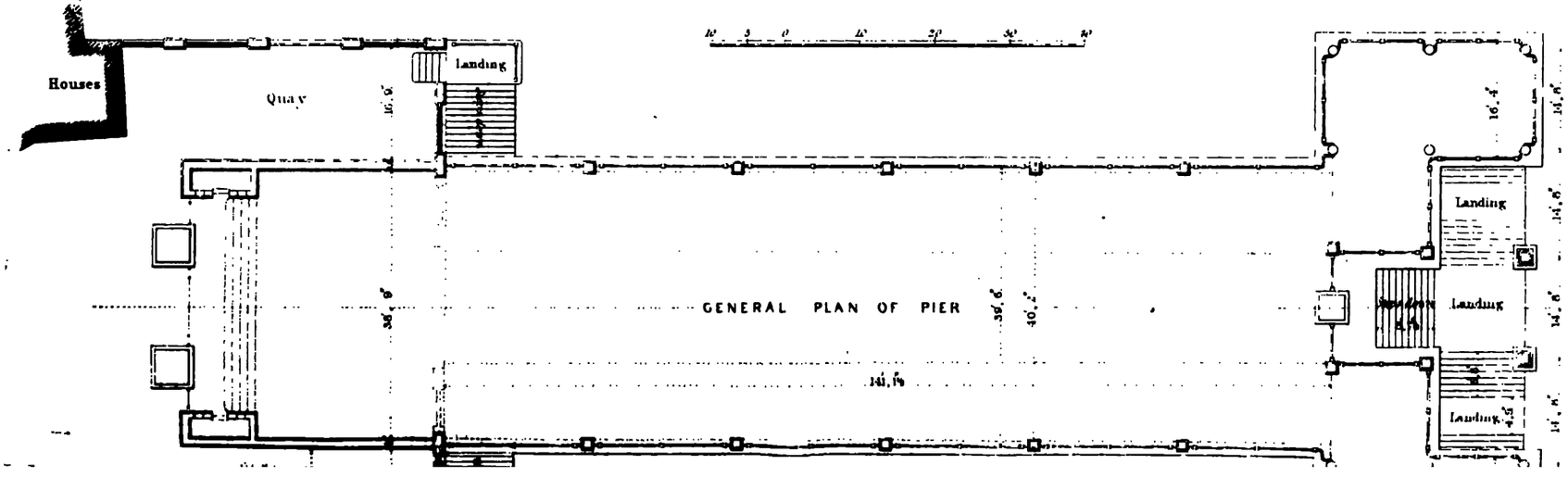
GENERAL PLAN OF PIER

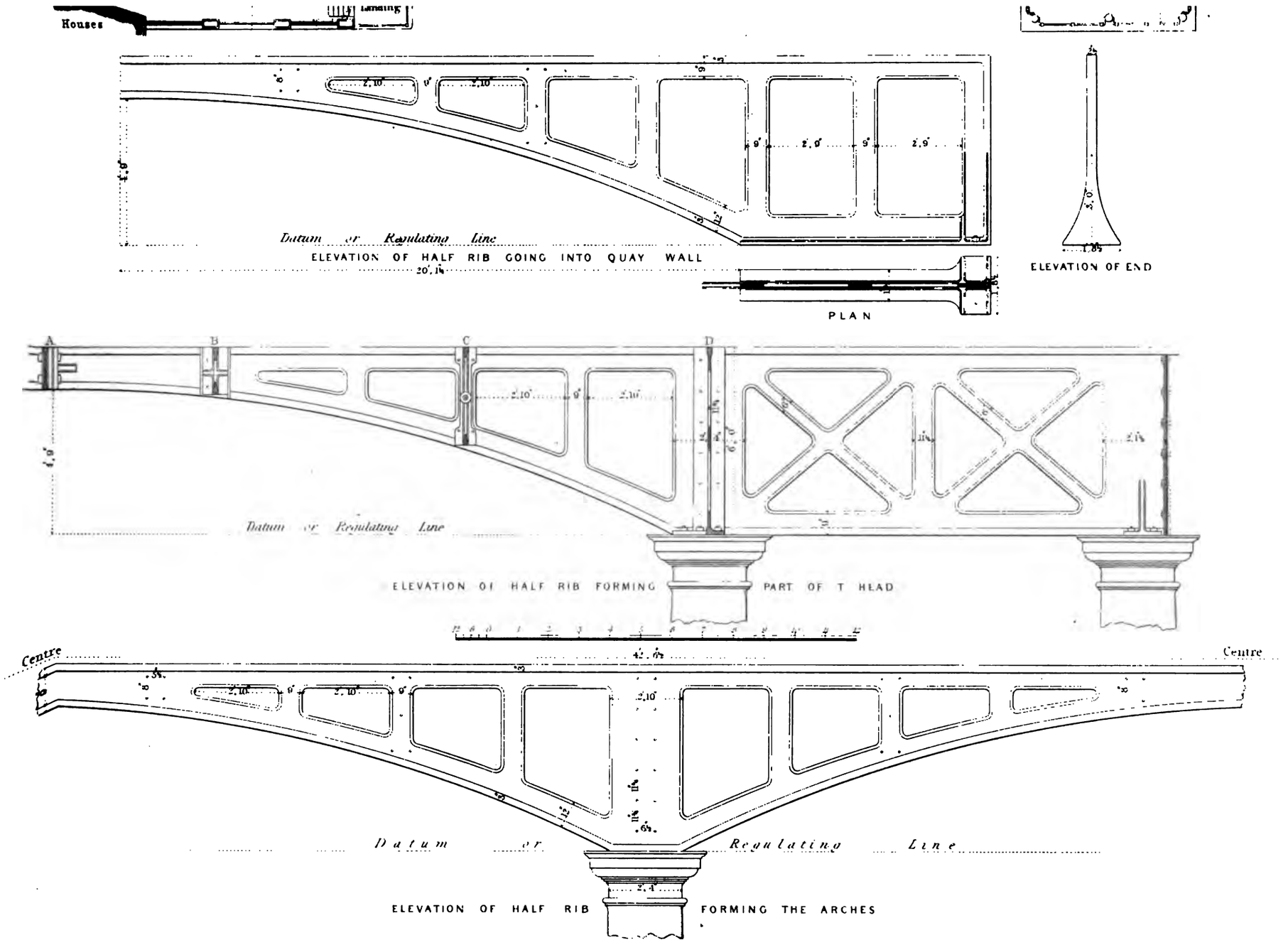

Datum or Regulating Line
ELEVATION OF HALF RIB GOING INTO GRAY WALL
PLAN
Datum or Regulating Line
ELEVATION OF HALF RIB FORMING PART OF Y HEAD
Centre
Datum or Regulating Line
ELEVATION OF HALF RIB FORMING THE ARCHES

GRAVESEND PIER.

W. TIERNEY CLARK







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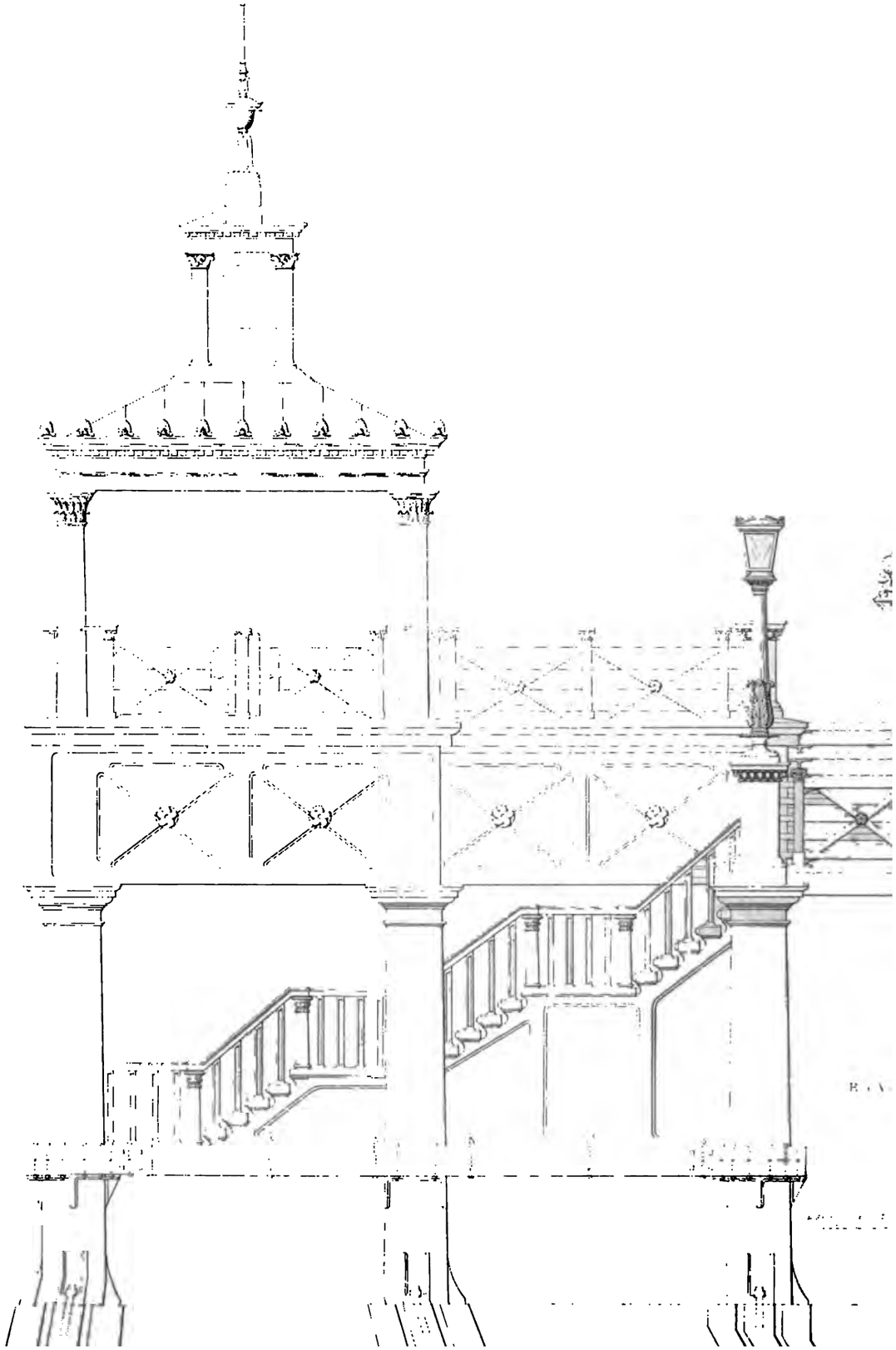
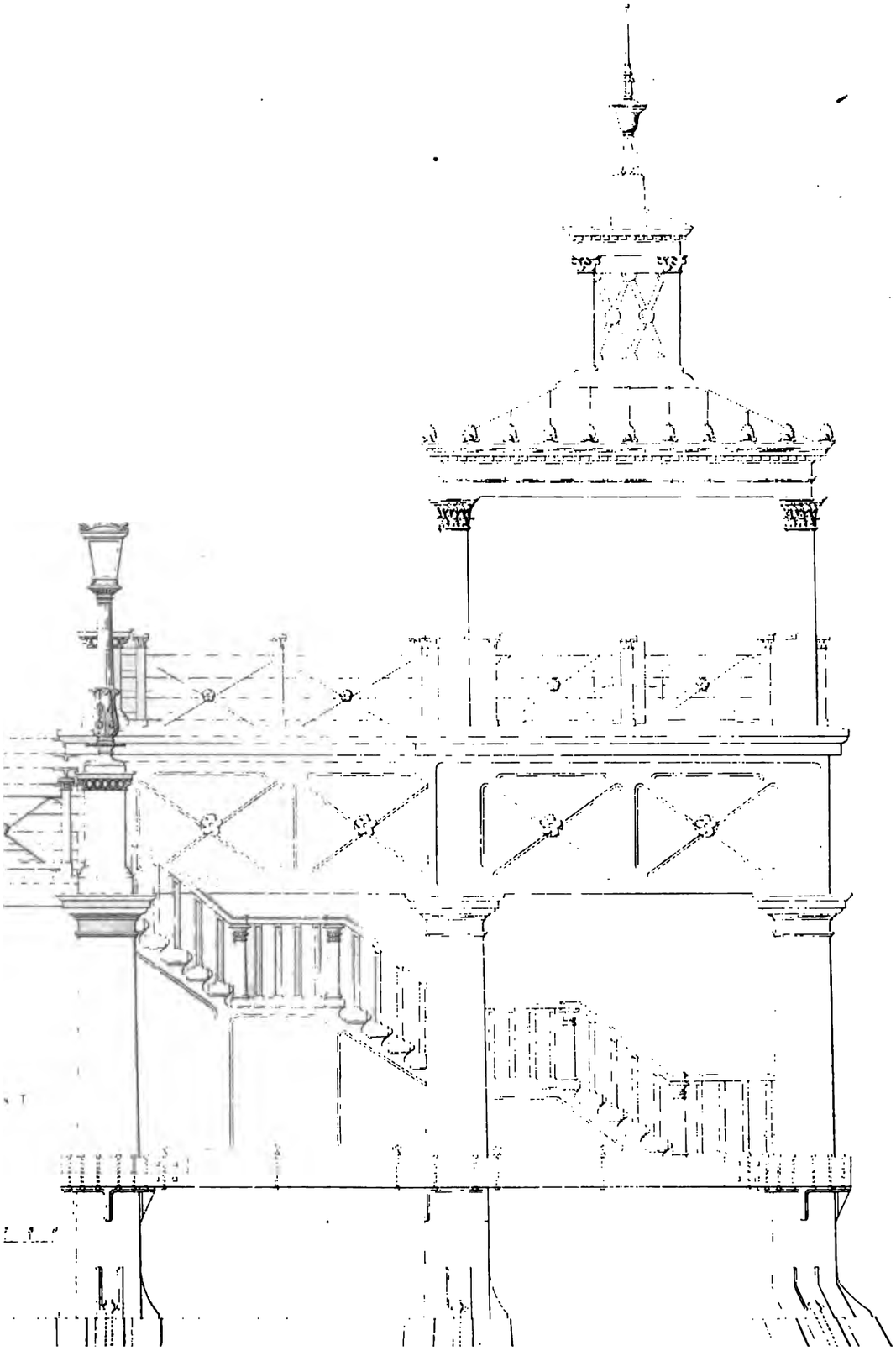
J.W. Lowry, sculp.





GRAVESEND PIER,

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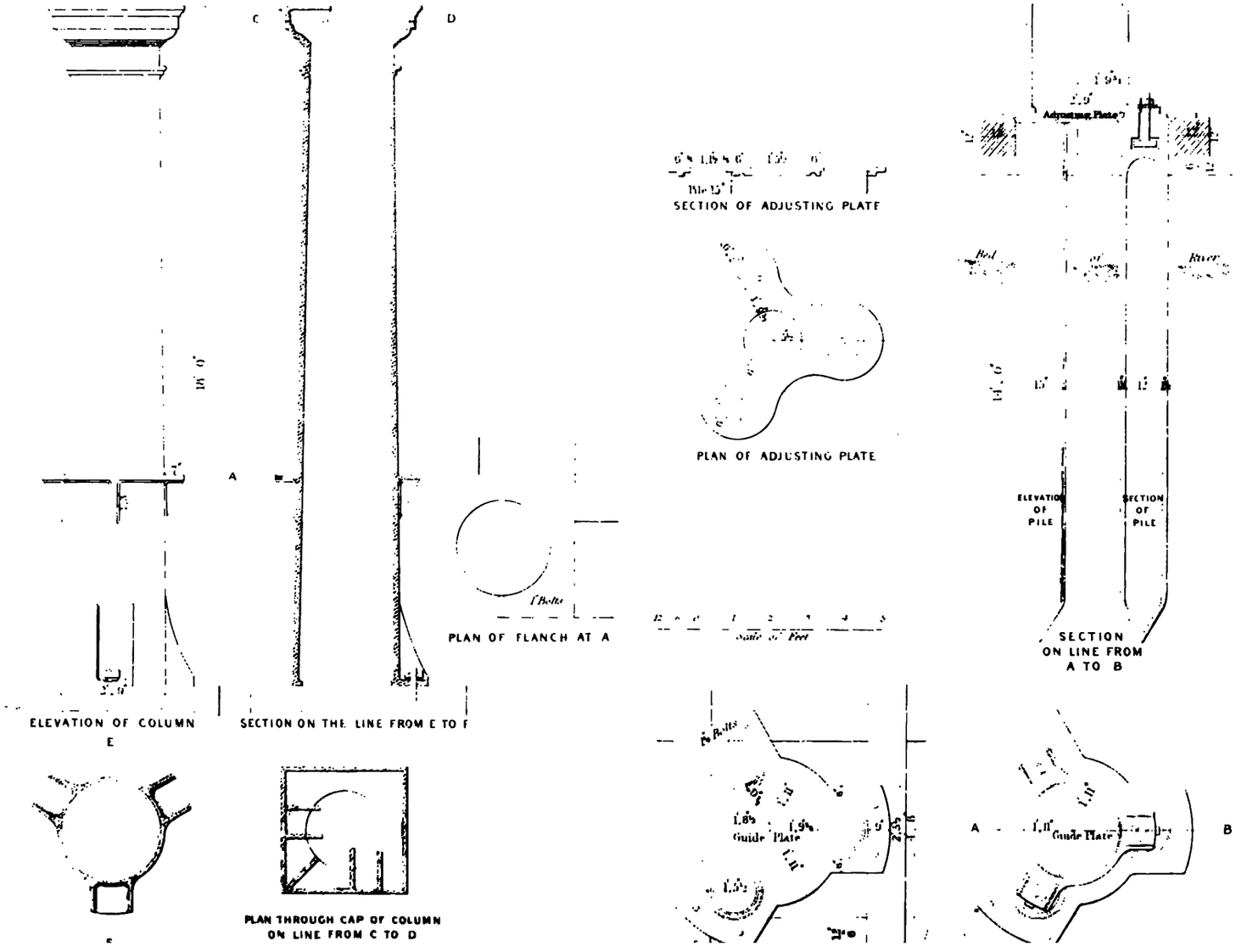
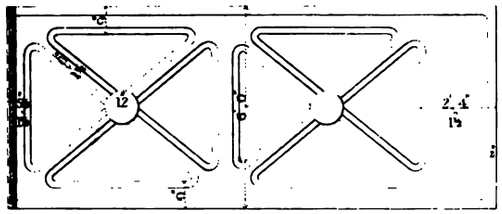
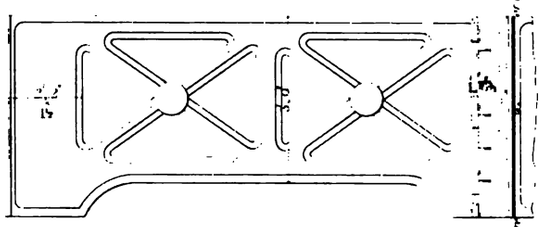


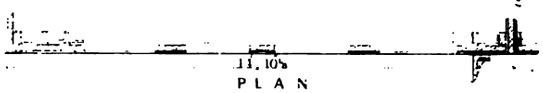


ELEVATION OF BEARING BEAM MARKED A DRAWING Nº 6

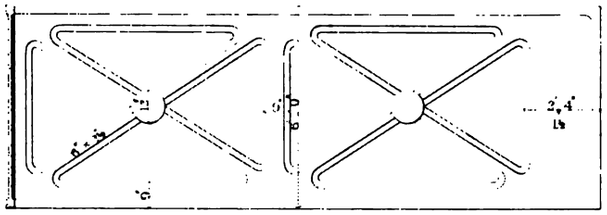
PLAN



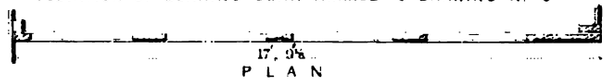
ELEVATION OF BEARING BEAM MARKED B DRAWING Nº 6



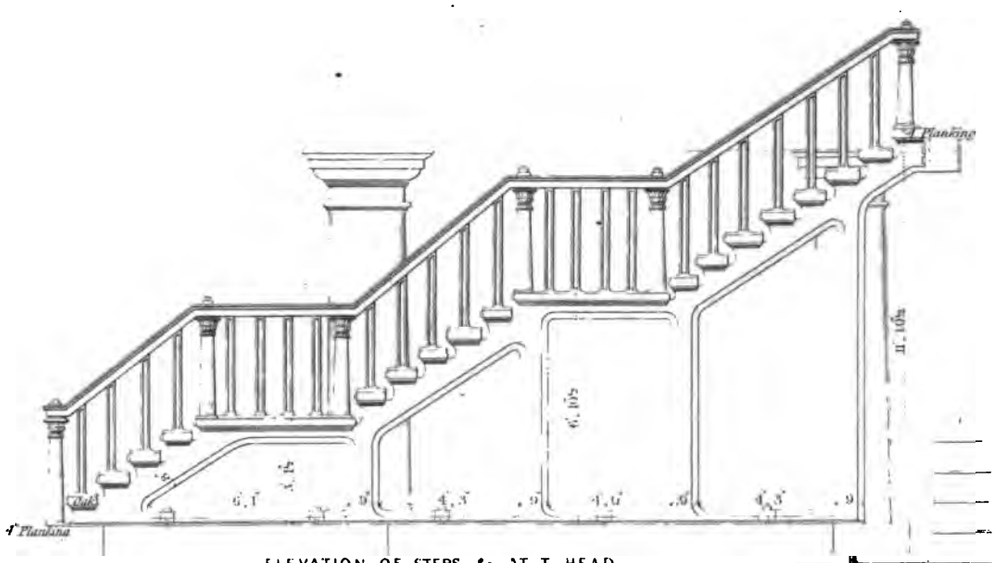
PLAN



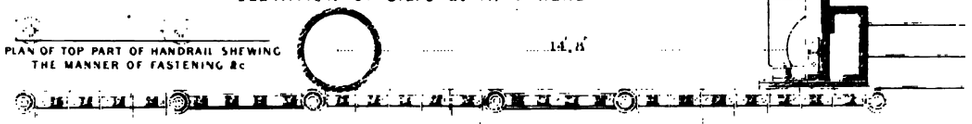
ELEVATION OF BEARING BEAM MARKED C DRAWING Nº 6



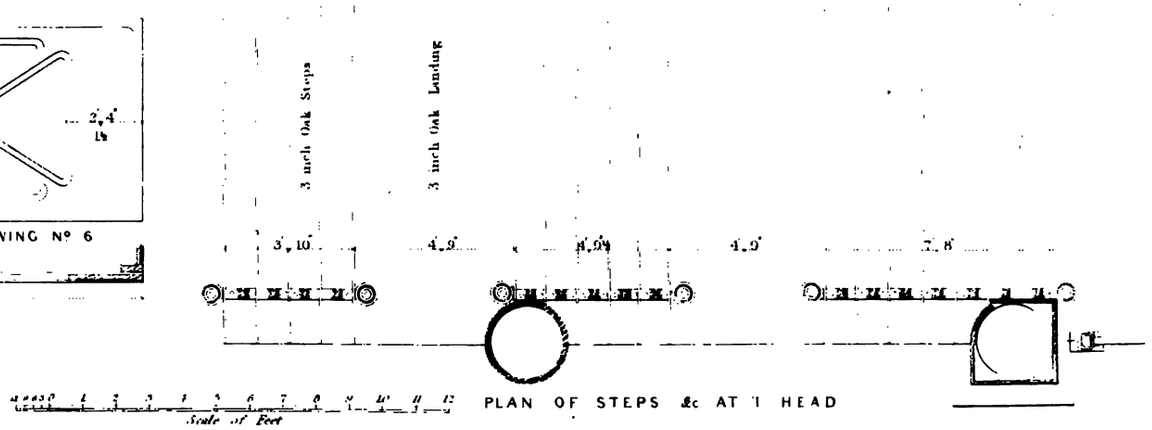
PLAN



ELEVATION OF STEPS &c AT T HEAD



PLAN OF TOP PART OF HANDRAIL SHEWING THE MANNER OF FASTENING &c



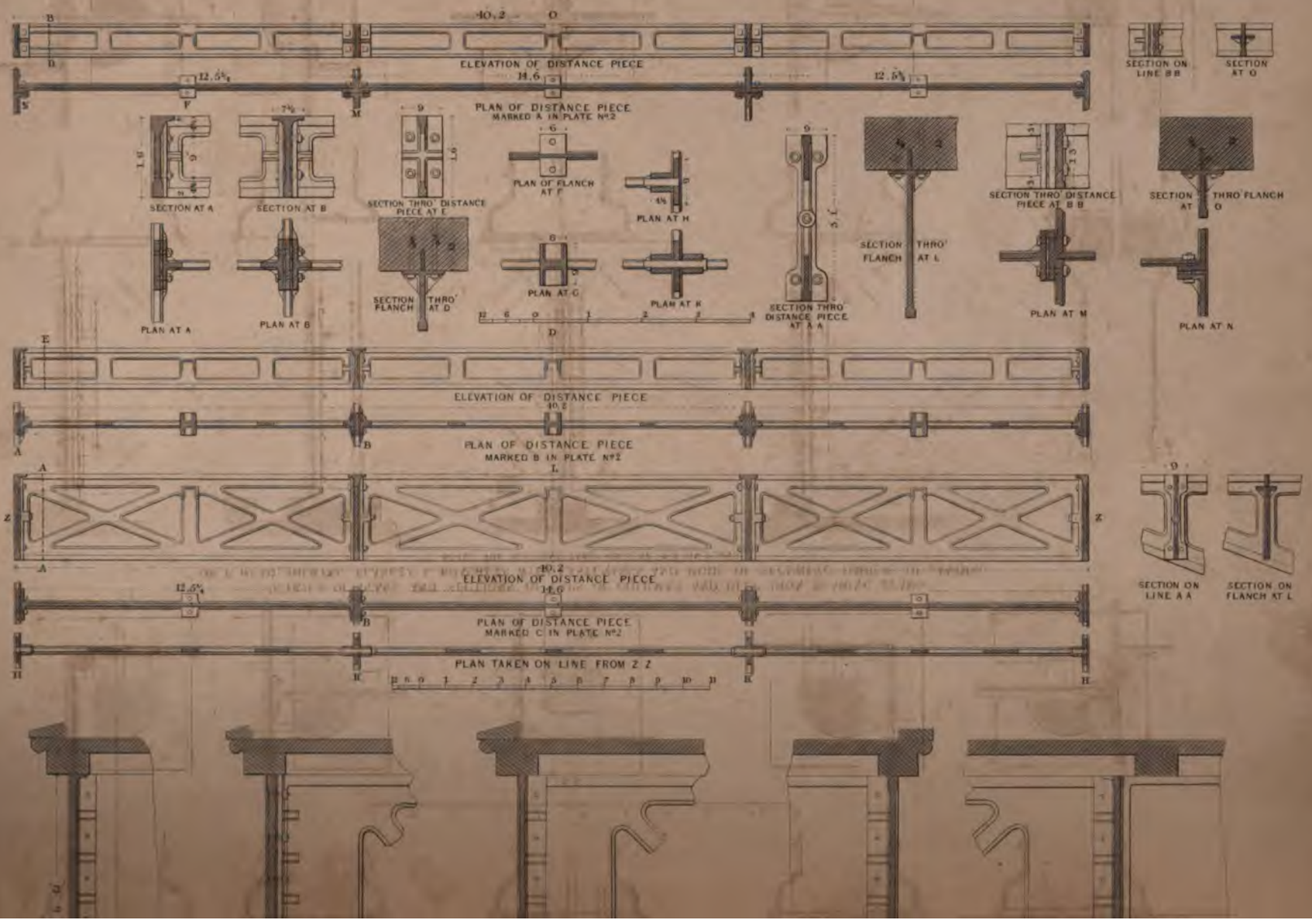
PLAN OF STEPS &c AT T HEAD

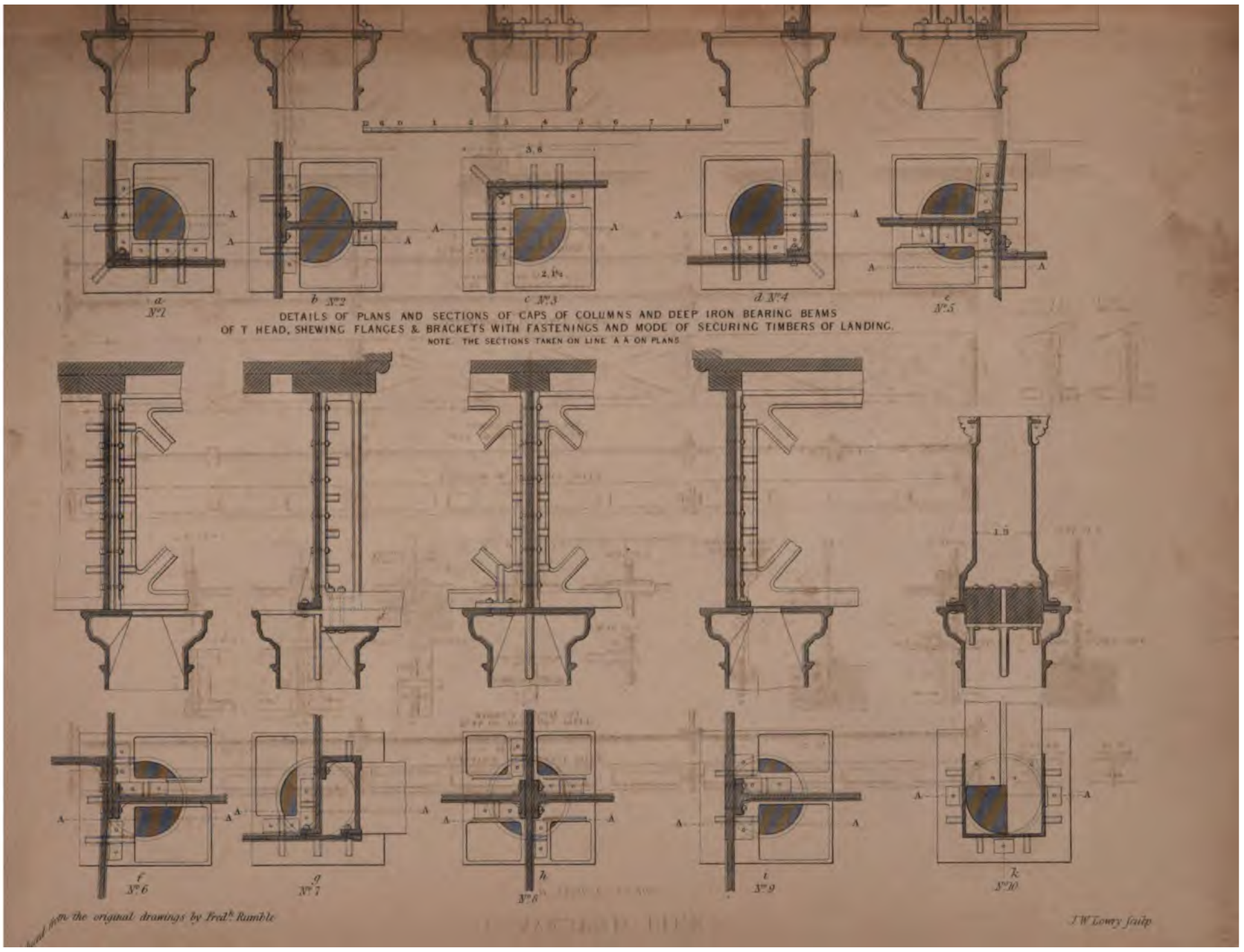
from the original drawings by Fred^k Rumble.

S. Bellin sculp.

GRAVESEND PIER.

W. TIERNEY CLARK.





DETAILS OF PLANS AND SECTIONS OF CAPS OF COLUMNS AND DEEP IRON BEARING BEAMS
OF T HEAD, SHEWING FLANGES & BRACKETS WITH FASTENINGS AND MODE OF SECURING TIMBERS OF LANDING.
NOTE. THE SECTIONS TAKEN ON LINE A A ON PLANS

...ed from the original drawings by Fred^k Rumble

J W Lowry faulp

Eng.

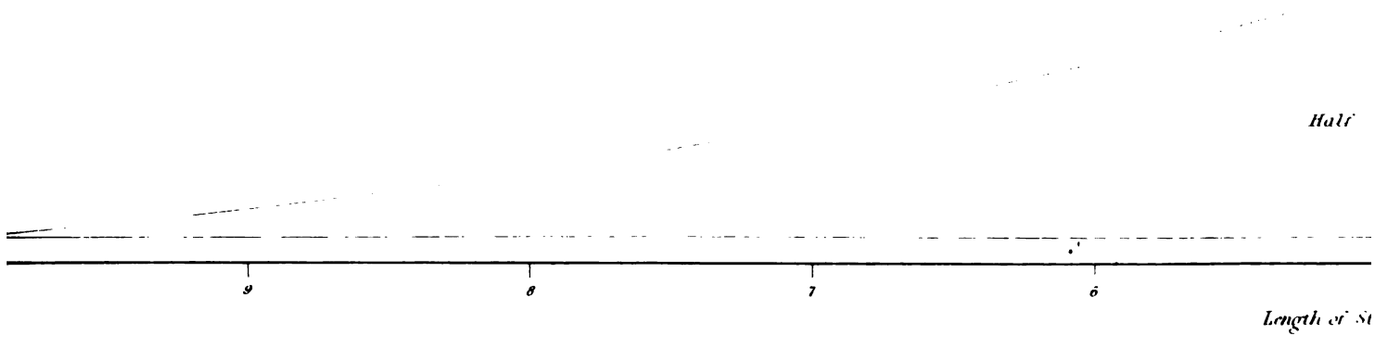
INDICAT(

FI

HUEL TO

ENLARGED FROM THE ORIGINAL VID

STEAM IN THE BOILE



The action of the Indicator in tracing a diagram is too well known to require description. In computing all Mr. Henwood's diag stroke forms no part of the power exerted during the working stroke.*

The working stroke of the engine terminated at C, when the equilibrium valve was opened. The steam's elasticity in the cylinde filling the space beneath the piston, and the pipe of communication, whilst the piston ascended in equilibrio. The equilibriu end of its course, when the exhausting valve was opened. The steam's elastic force above the piston being then virtually increa the quantity of steam recovered, & forming the cushion, at an elasticity of 10.7 lbs. per sq. in. After a rest of 4.8 sec.^{ds} nearly 19^{lbs} per sq. in. The working stroke then commenced, the steam arriving at its maximum elasticity at B when the until the closing of the valve. This operation is not instantaneous, being effected by the descent of the plug rod. The diagr quarter of the stroke. *Vide Henwood. Trans. Inst. C. E. Vol 2. p. 49.

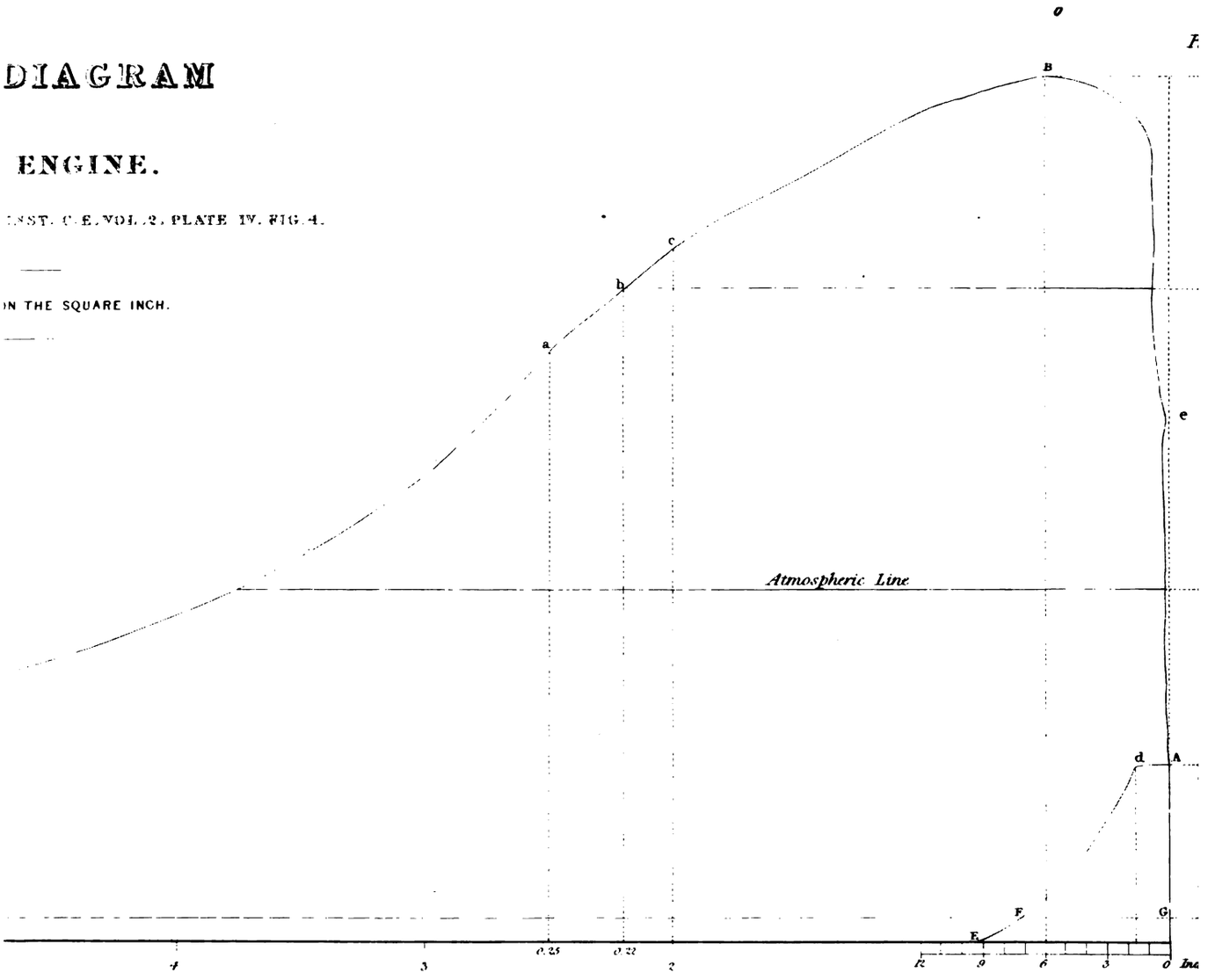
h. Parkes del.

DIAGRAM

ENGINE.

INST. C.E. VOL. 2. PLATE IV. FIG. 4.

IN THE SQUARE INCH.



pressures must be taken above the line C G, *as the quantity of expansion suffered by the steam during the return*

7lbs per sq. in. The space C D E F *represents the amount of the steam's expansion during the return stroke, caused by its was closed at* E. *The steam's compression then commenced, and continued till the piston arrived at* d, *within 1¾ in. of the vacuum formed beneath it, the return stroke nearly ceased, and the engine came finally to rest at* A. *The space* A F G *represents valve was opened by the cataract, but the engine's motion did not ensue till the steam attained at* e, *an elastic force of ul descended 6 inches, from which period of the stroke the steam expanded with great rapidity, though not with uniformity r indicates that the closing of the valve commenced at* c, *and terminated at* a, *when the piston had passed through a*

S. Bellin faul

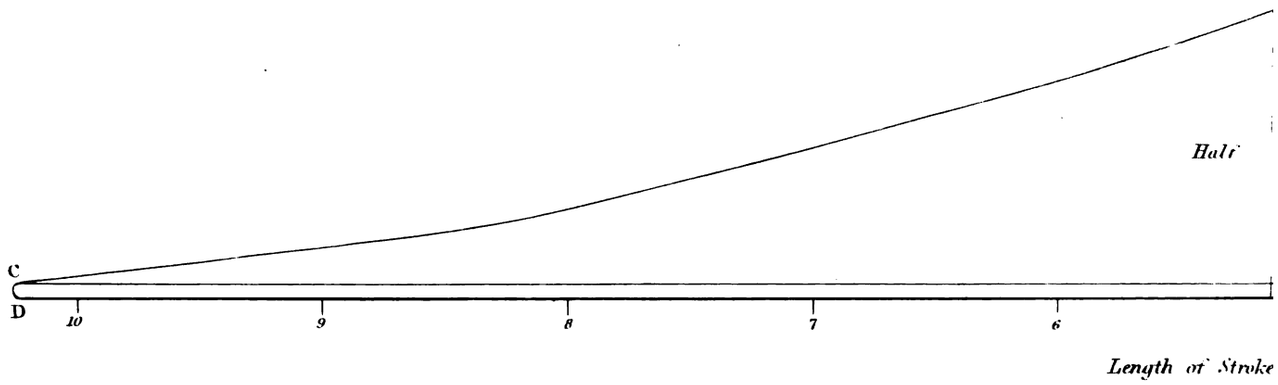
INDICATOR DIAG.

FROM THE

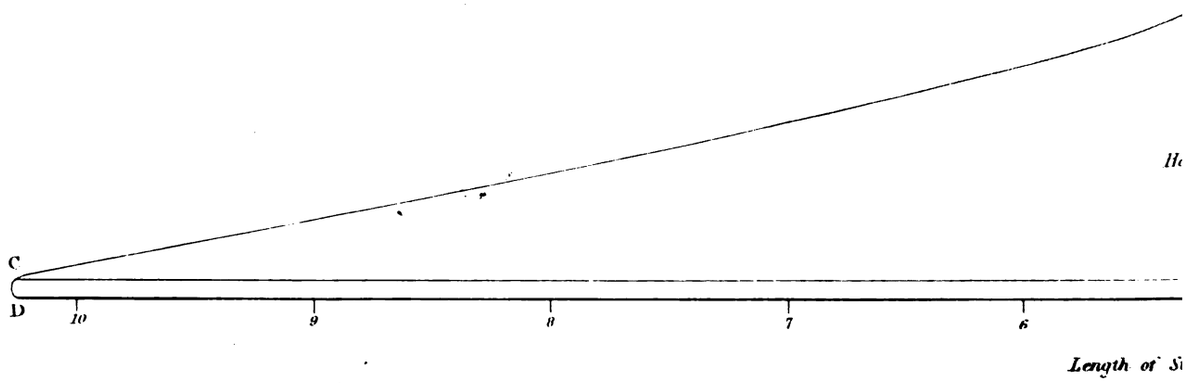
EAST CRINNIS ENGI

ENLARGED FROM THE ORIGINALS. VIDE TRANS. INST. C.E. VO

STEAM IN THE BOILERS 36.8 LBS ON THE SQUAR



STEAM IN THE BOILERS 26.3 LBS ON THE SQUARE



Vide explanation of the Hull Town Engi

S

E IV. FIGS. 8 & 9.

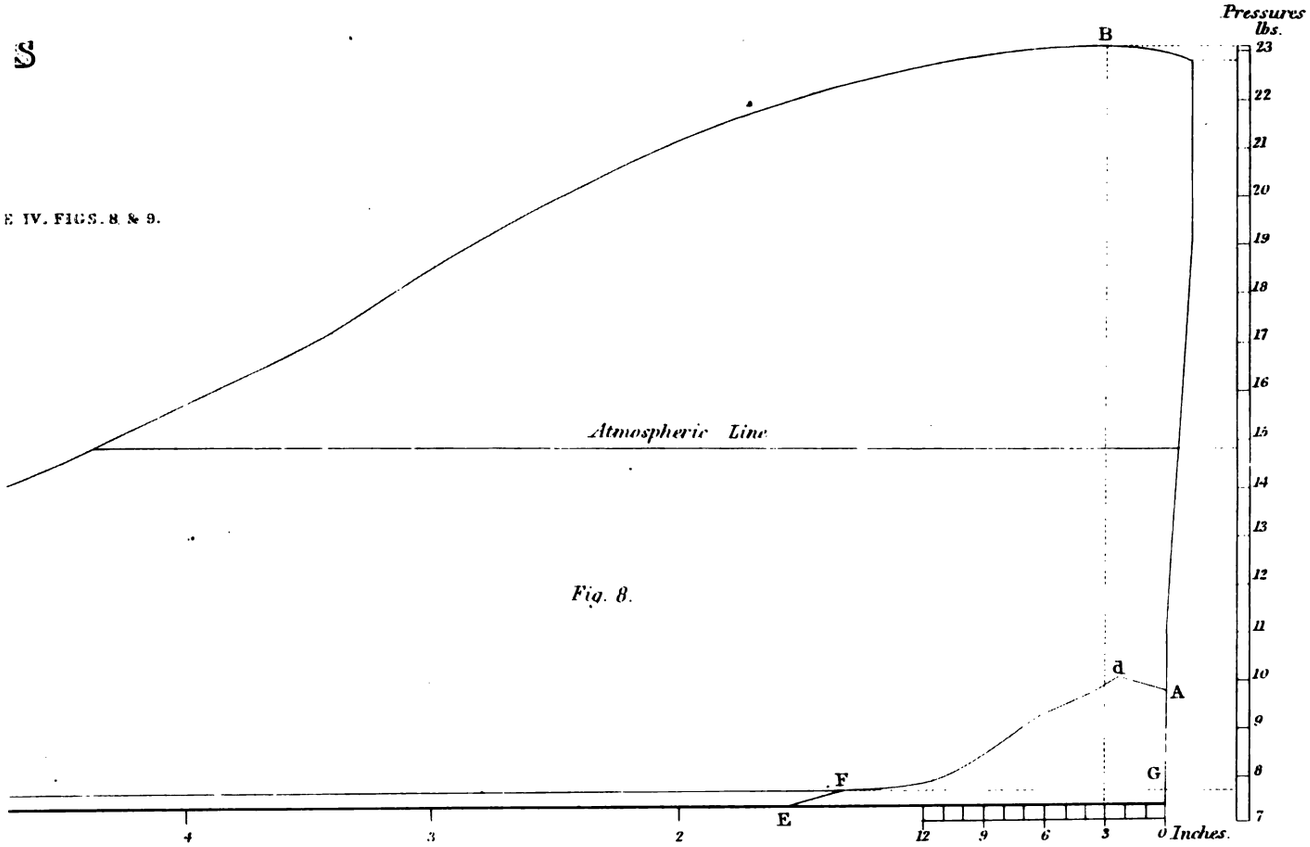


Fig. 8.

ches.

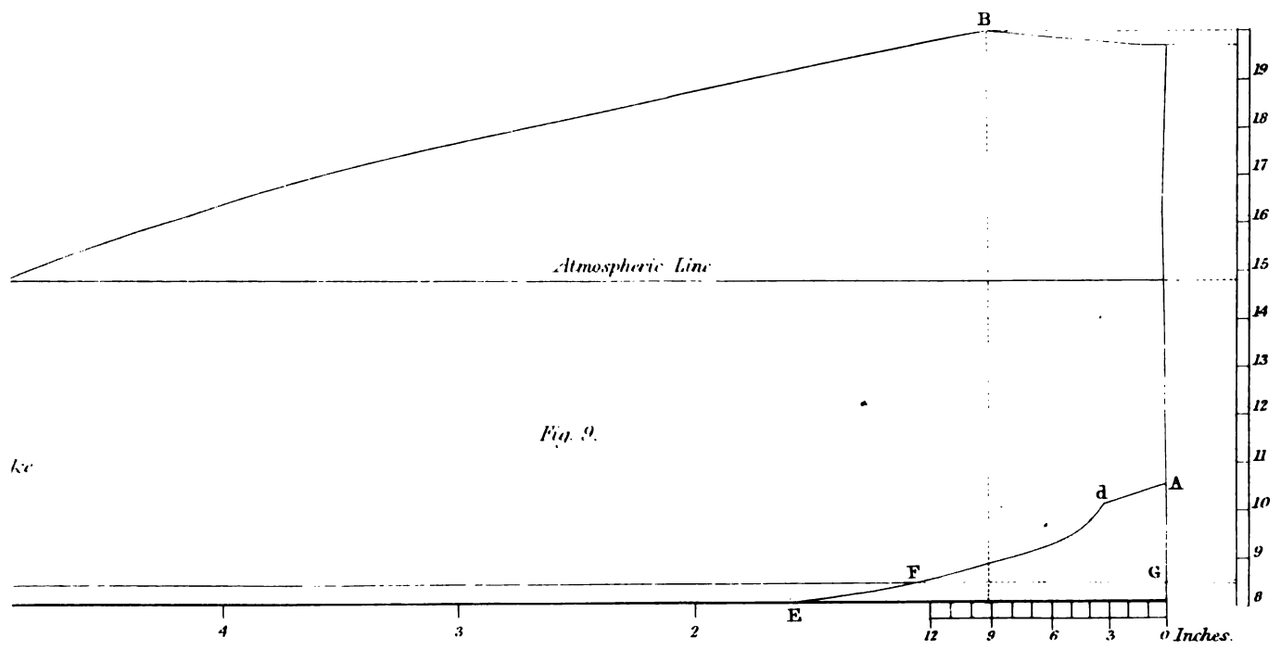


Fig. 9.

ke

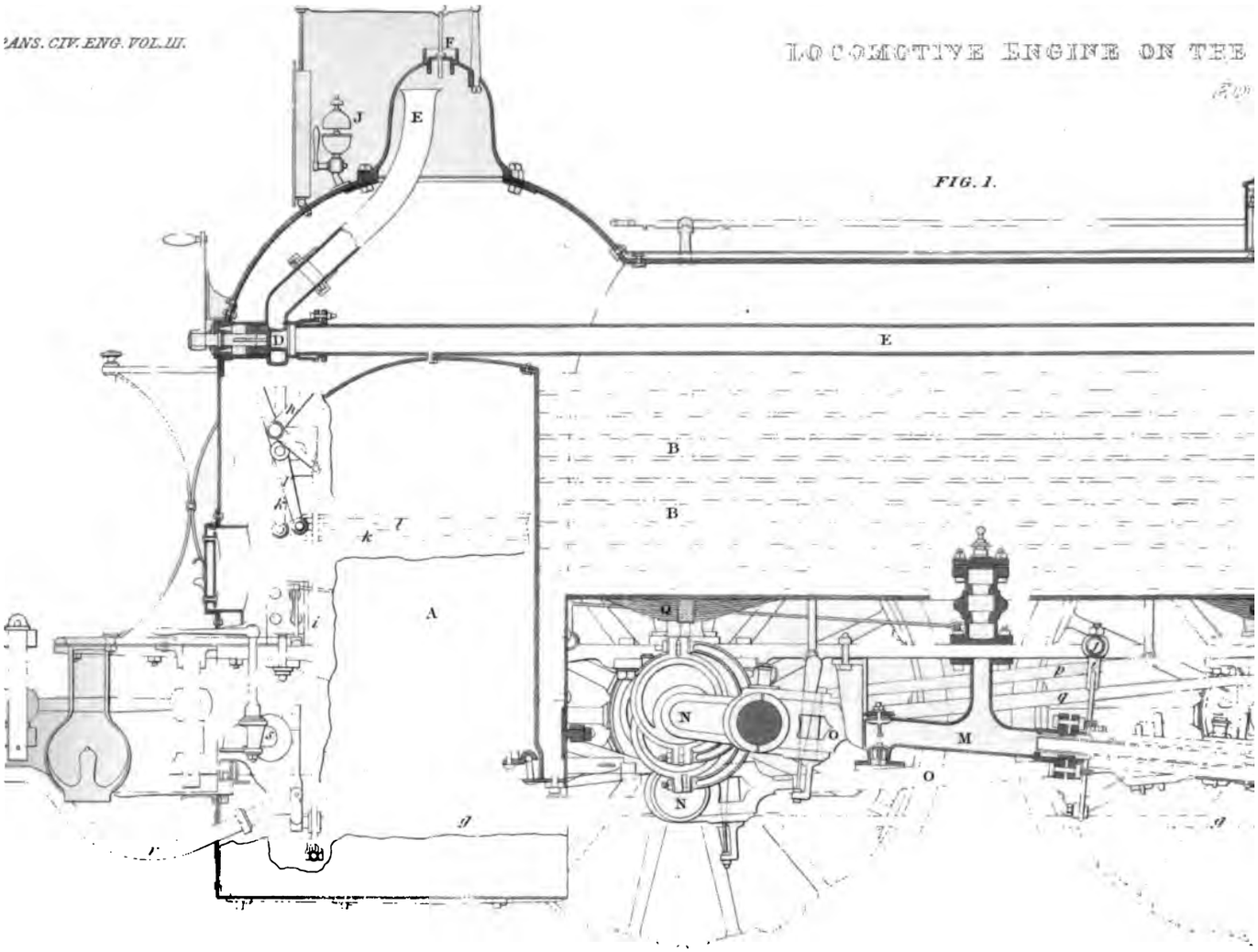
3 inches.

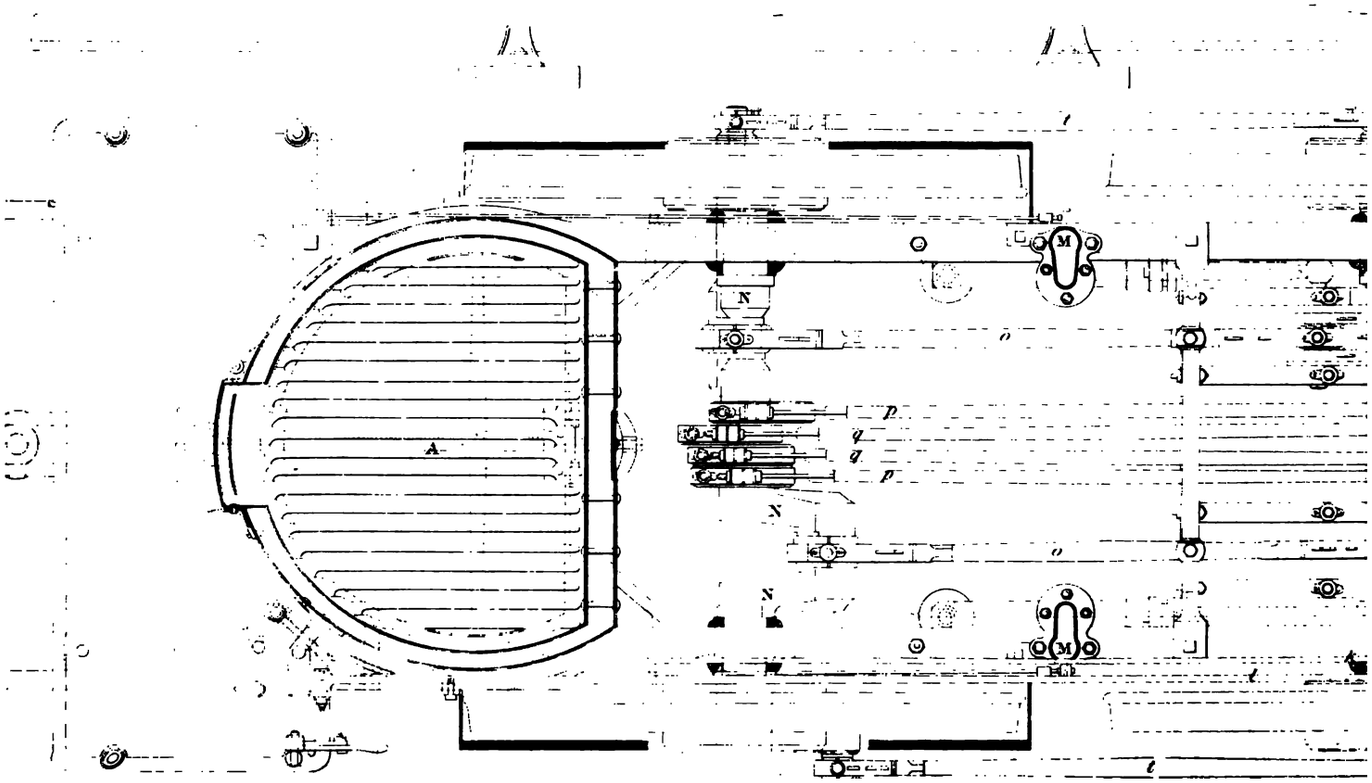
n. The letters refer to similar parts.

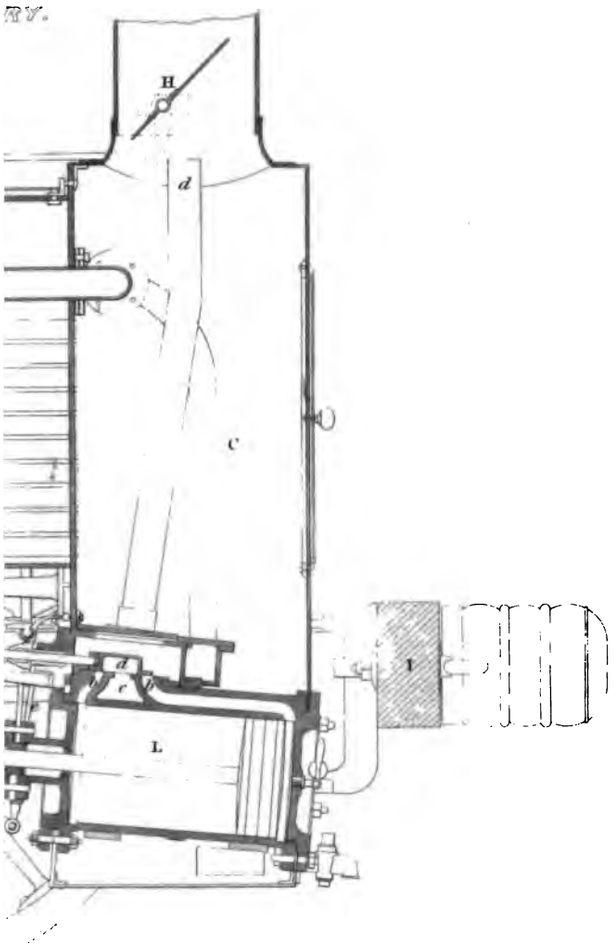
S. Bellin sculp.

LOCOMOTIVE ENGINE ON THE

FIG. 1.







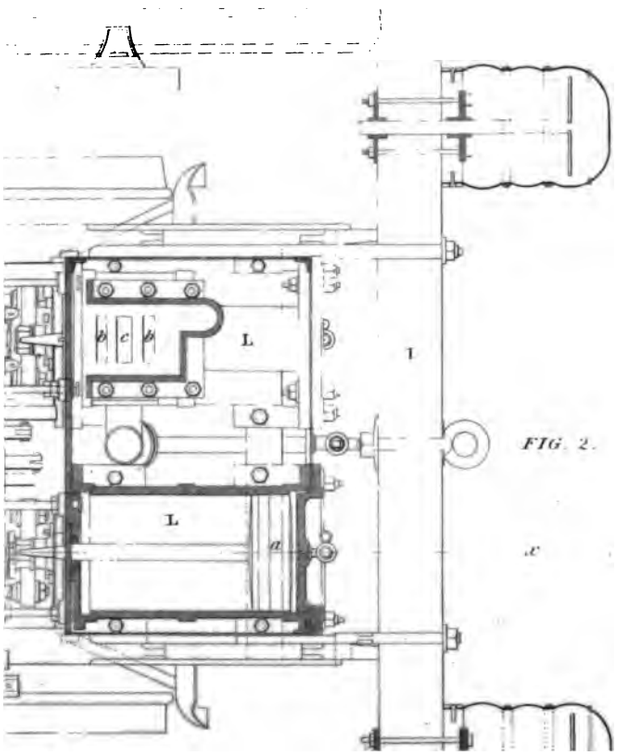


FIG. 2.

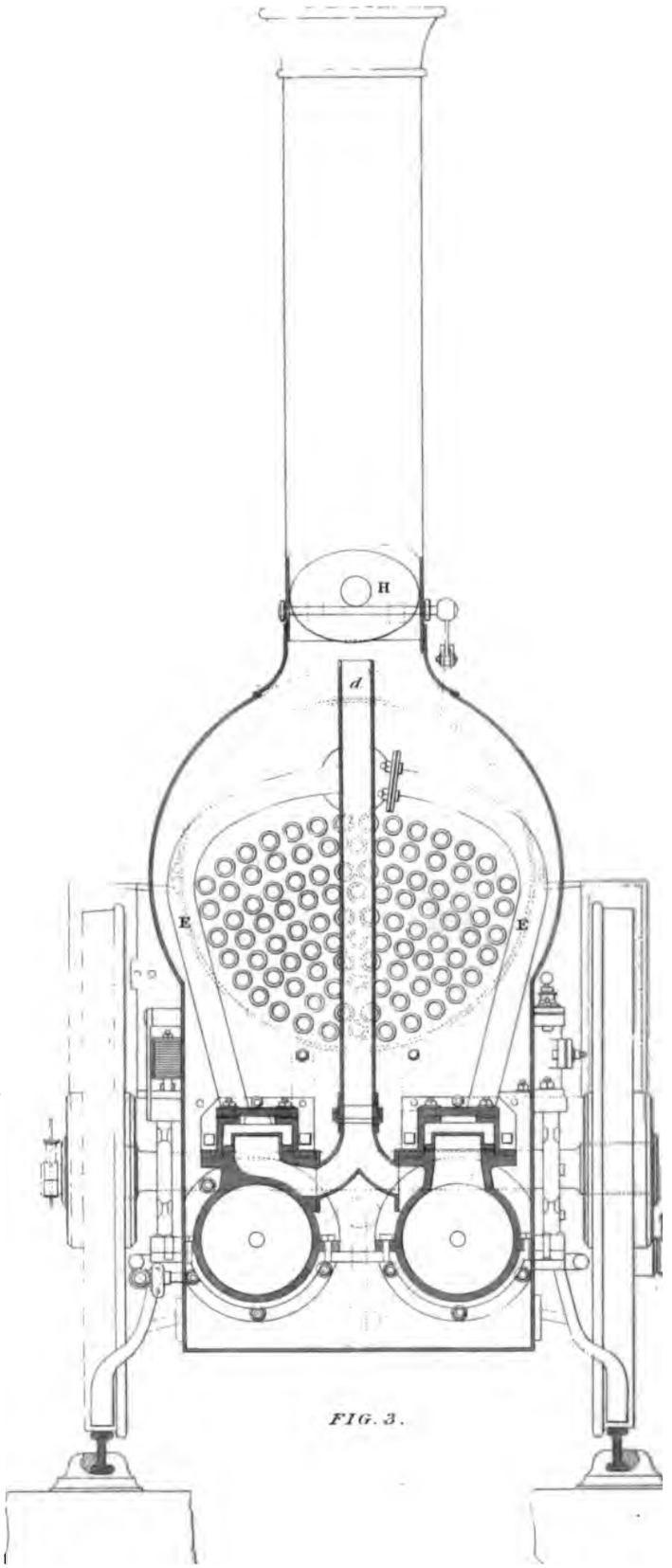
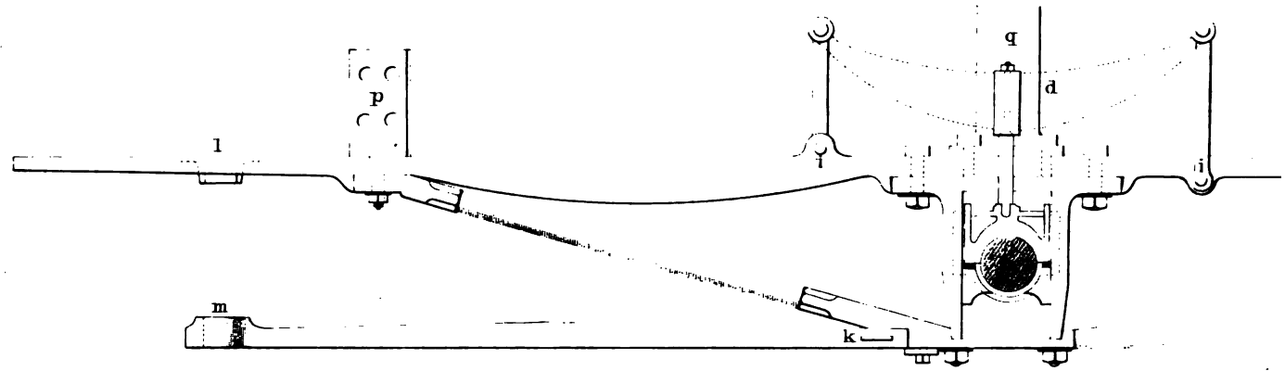
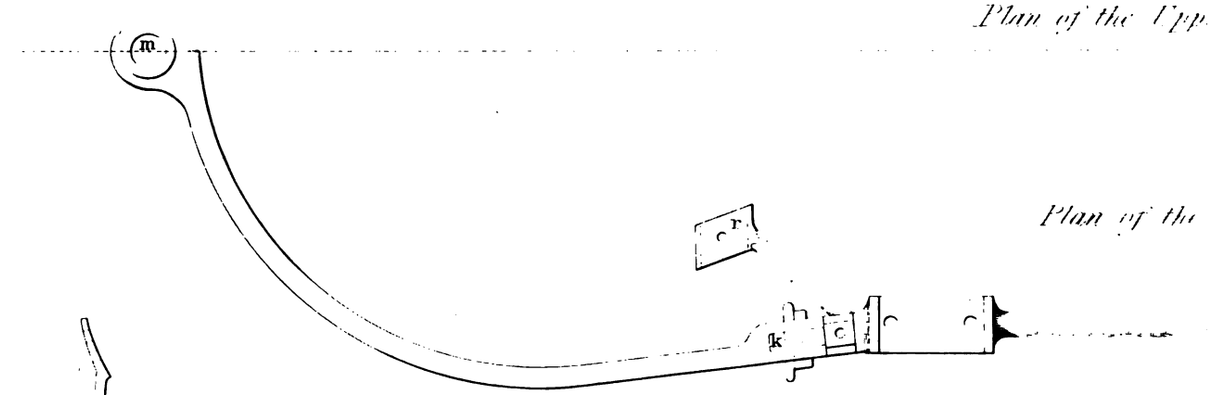


FIG. 3.

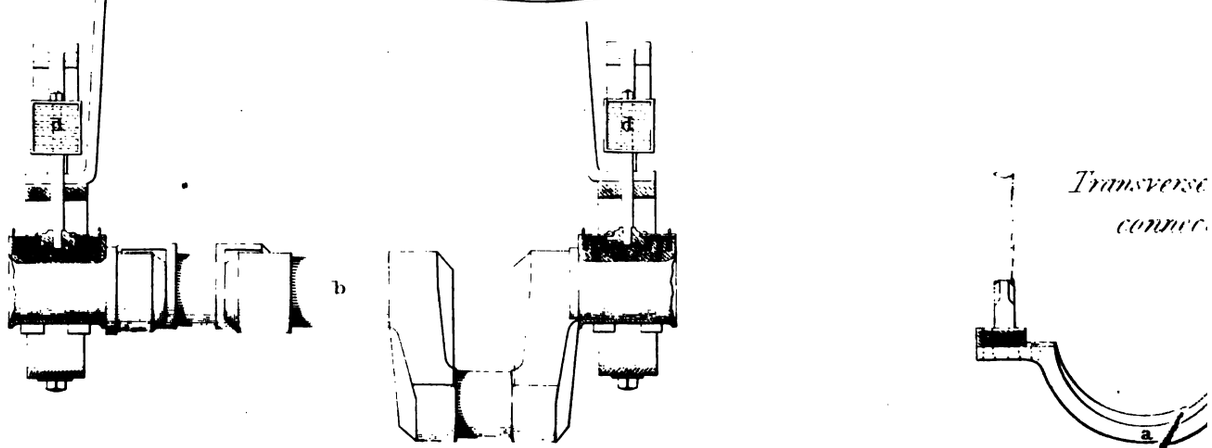


Elevation of

Plan of the Upp



Plan of the

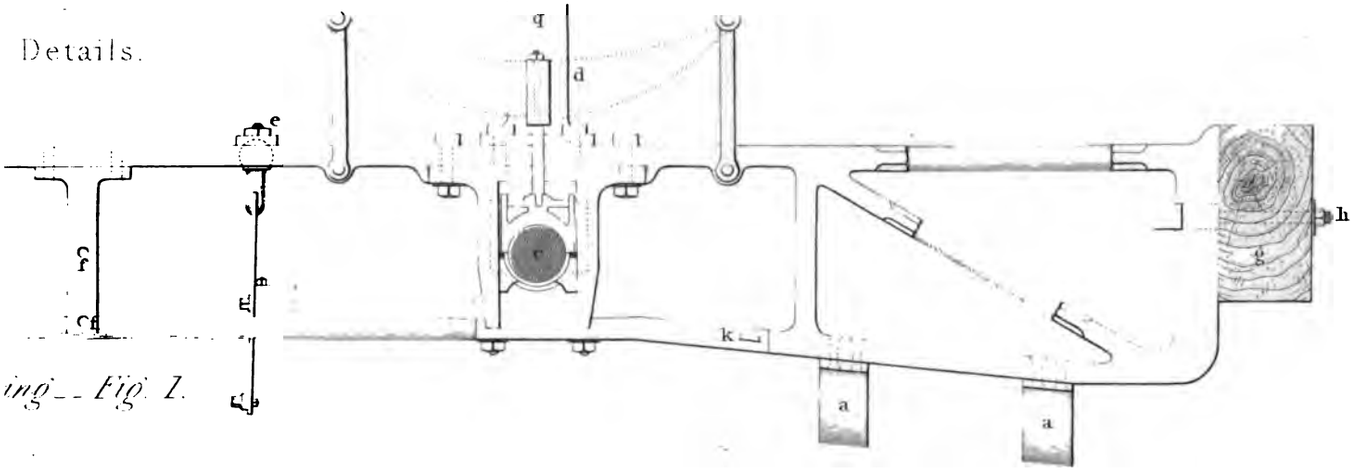


Transverse
connec

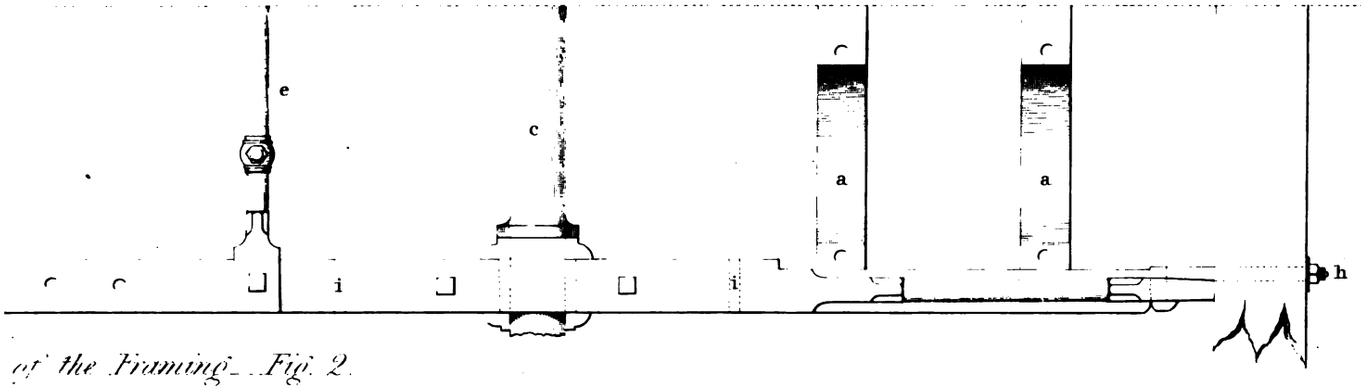
Section through the Frame, shewing the Crank Axle, Fig. 4.

12 9 6 3 0

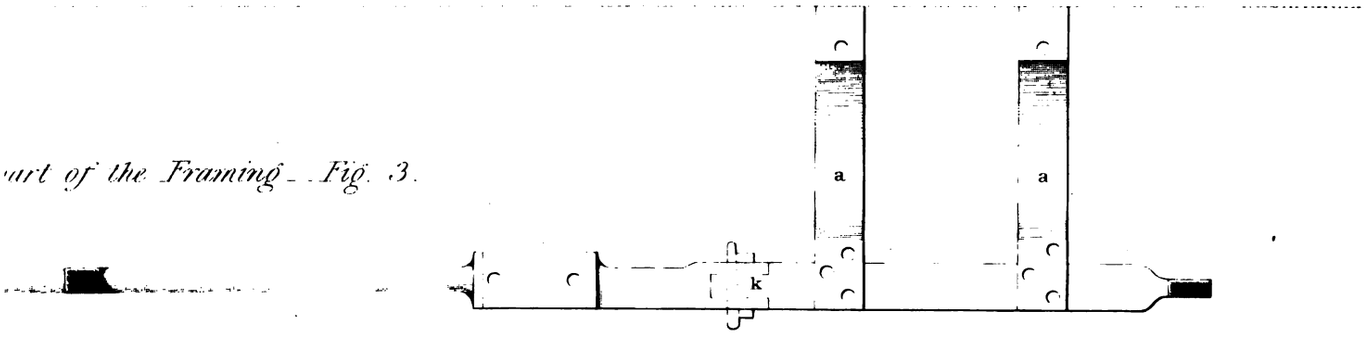
Details.



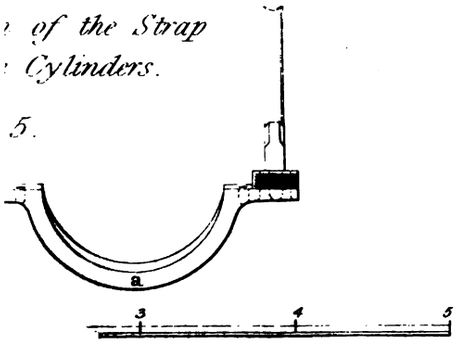
ing – Fig. 1.



of the Framing – Fig. 2.

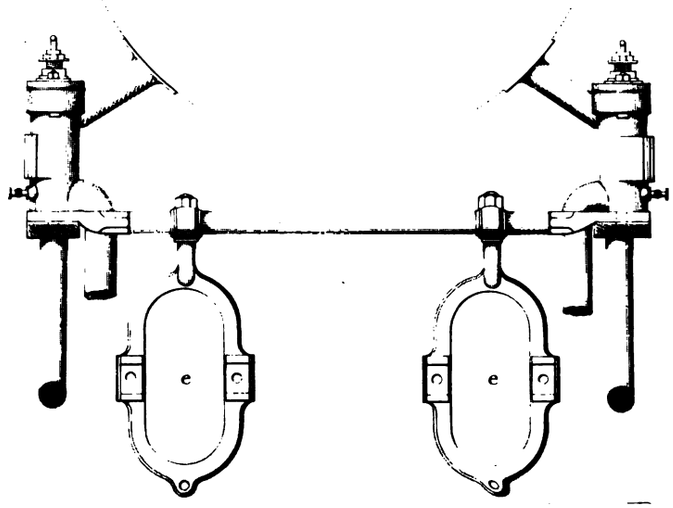


art of the Framing – Fig. 3.

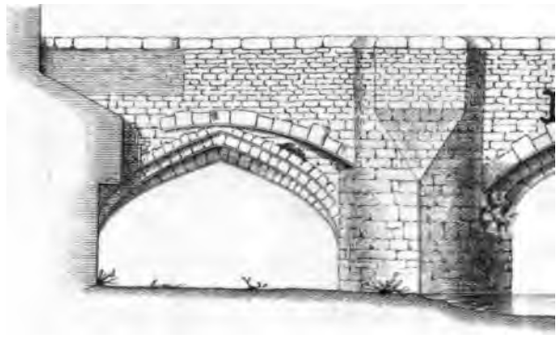


of the Strap
Cylinders.

5.



OLD

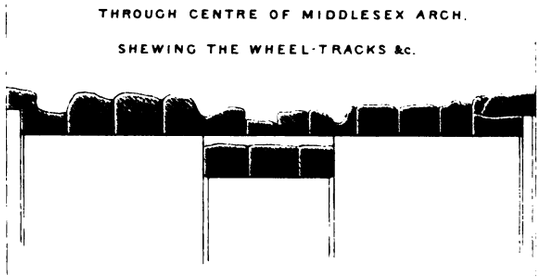


LON

SECTION

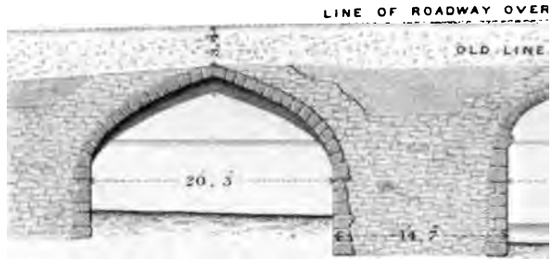
THROUGH CENTRE OF MIDDLESEX ARCH.

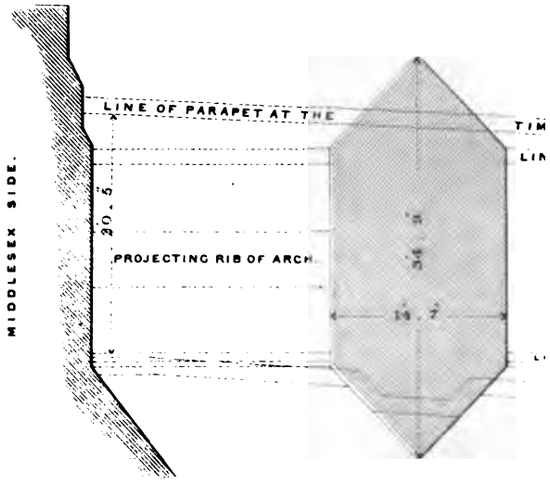
SHEWING THE WHEEL-TRACKS &c.



0 5 10

SCALE OF FEET.

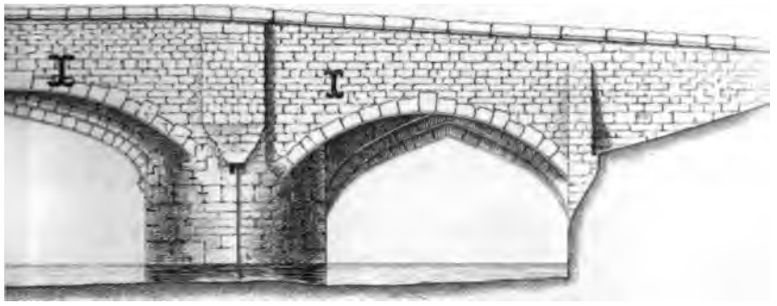




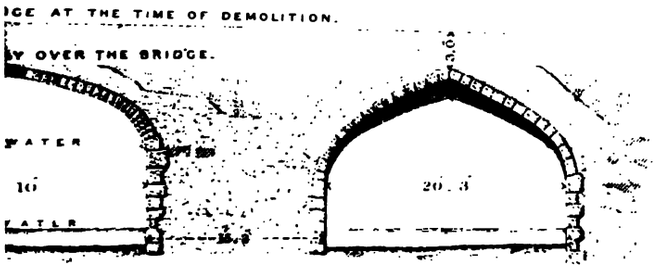
SCALE OF 10 5 0 10

Y BRIDGE.

ATION.



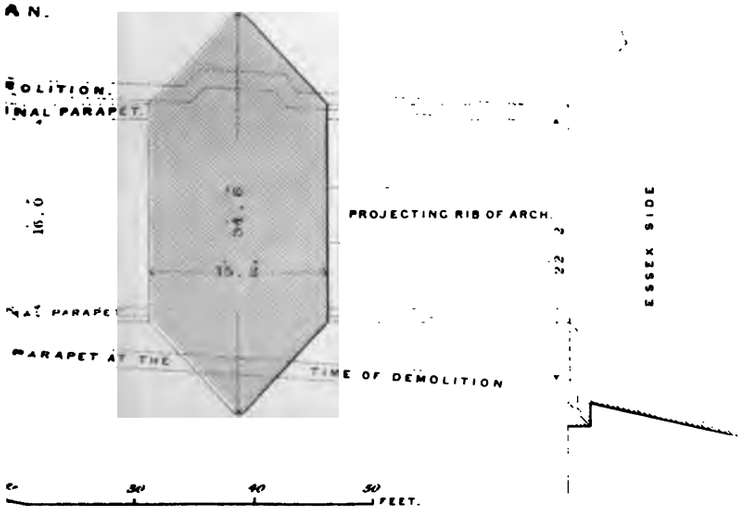
AL SECTION.



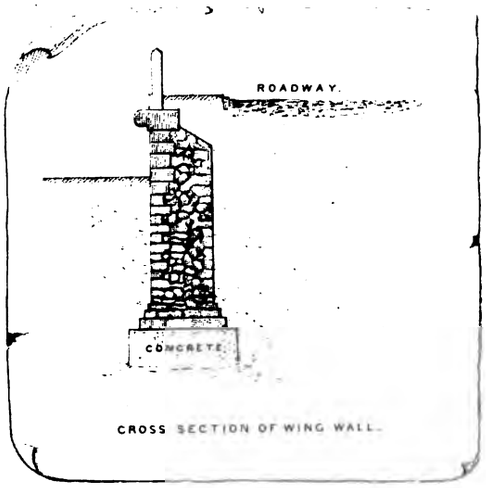
NORTH ELEVATION OF ESSEX ARCH

SECTION THROUGH ESSEX ARCH
SHEWING THE PROJECTING RIB

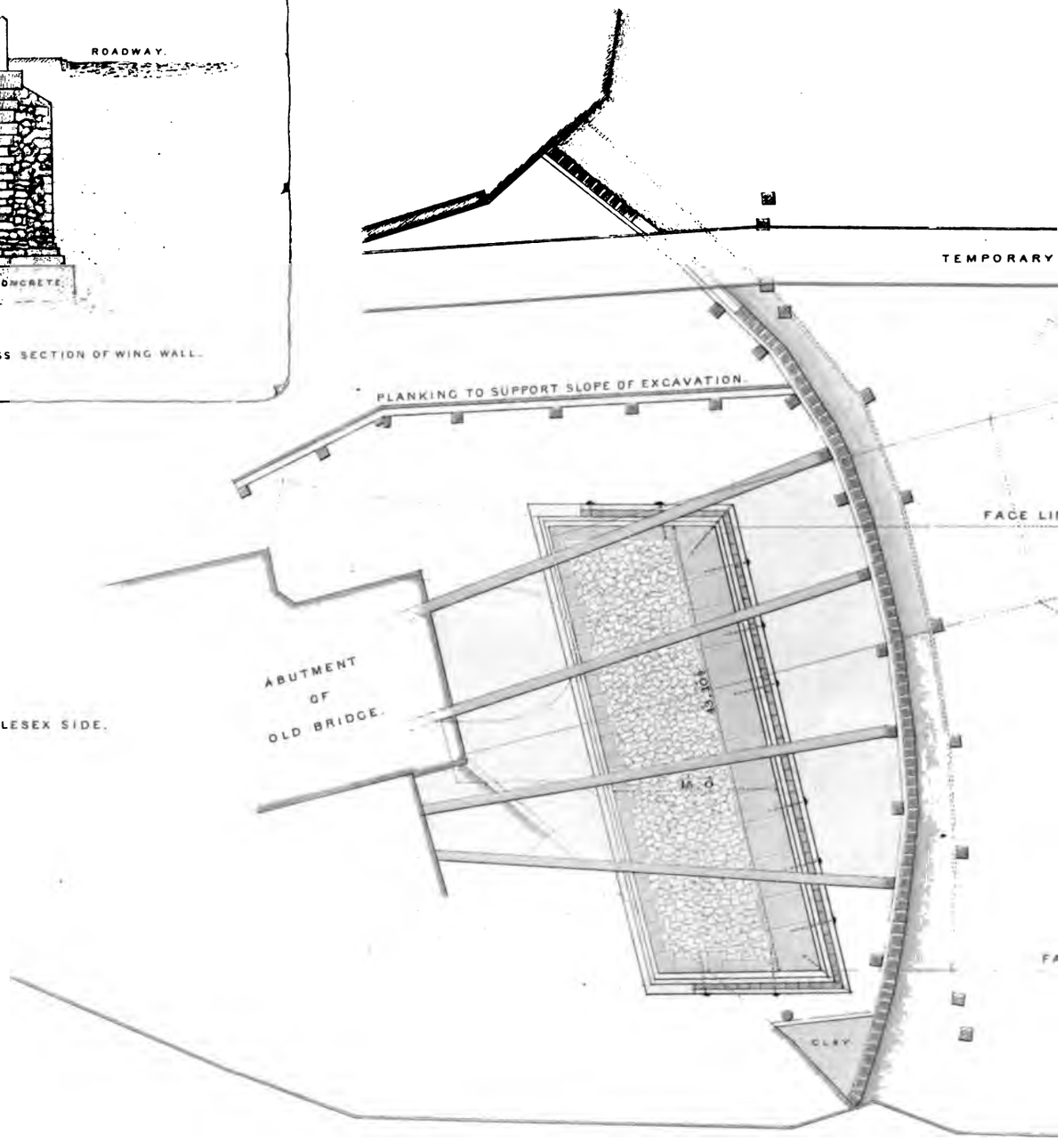
AN.



PLAN OF ABU

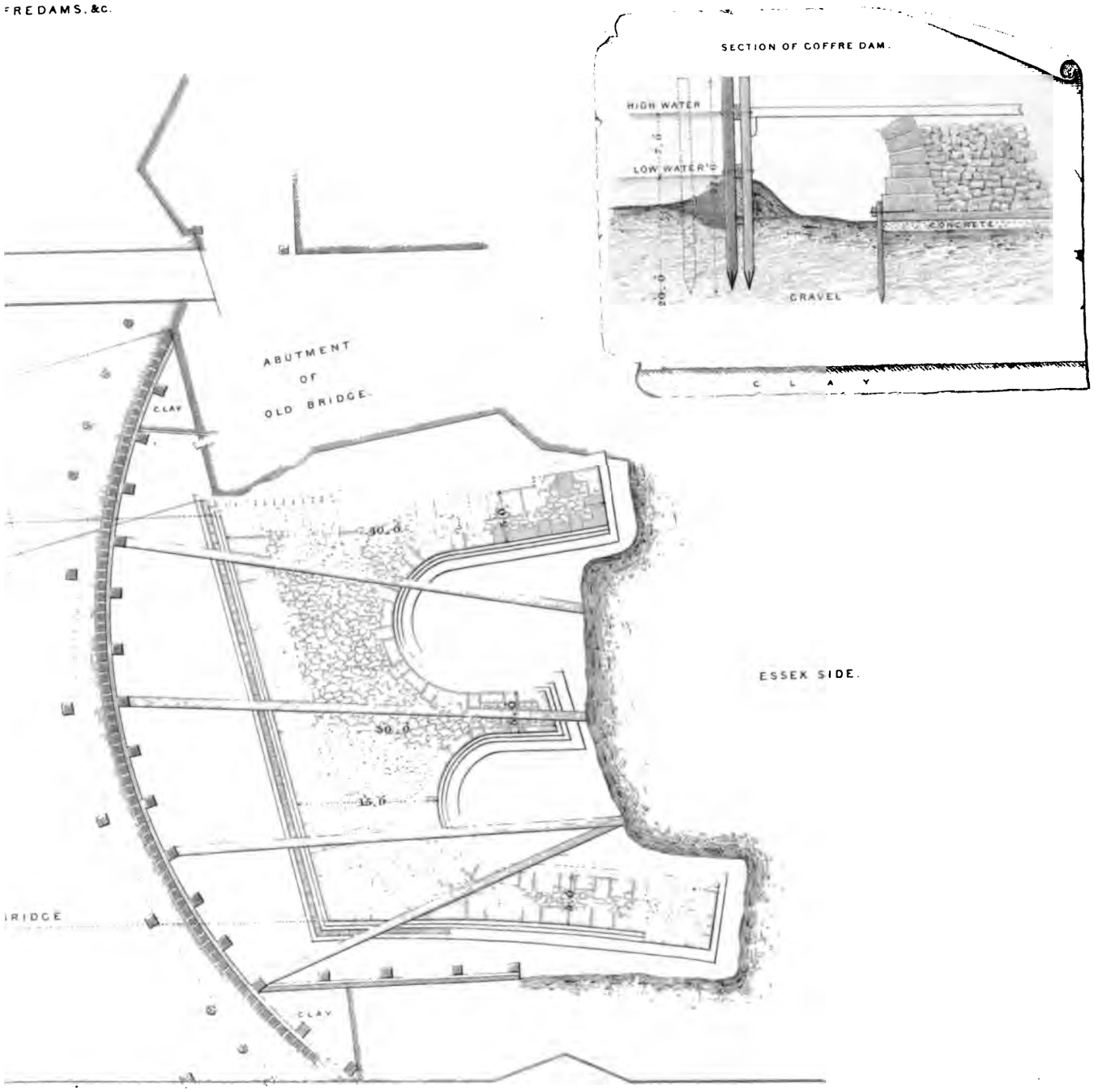


CROSS SECTION OF WING WALL.



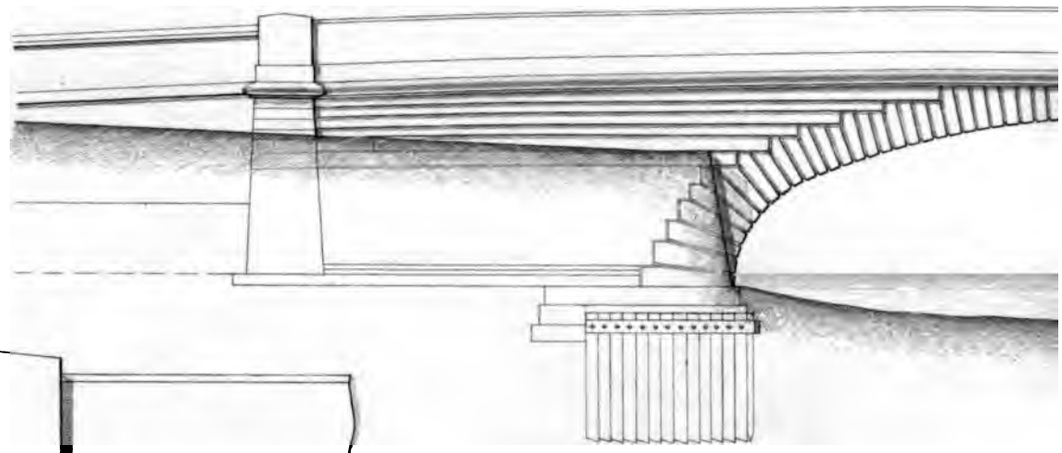
GE.

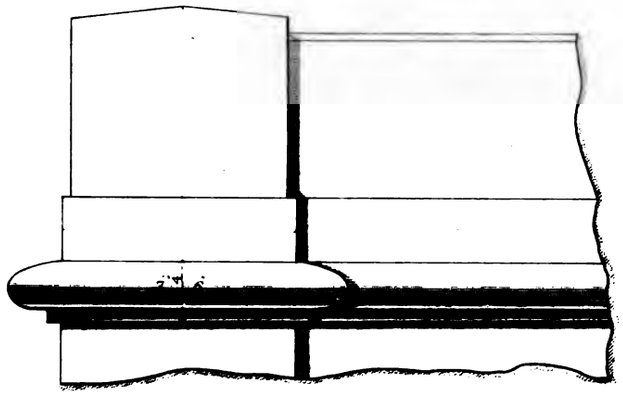
FREDAMS. &c.



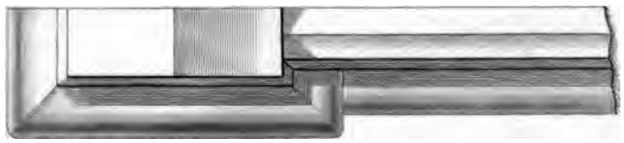
PORARY ROAD BRIDGE.

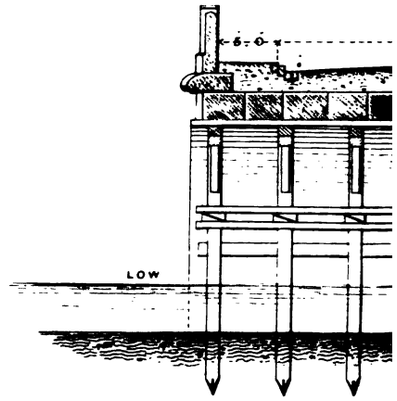
BOW



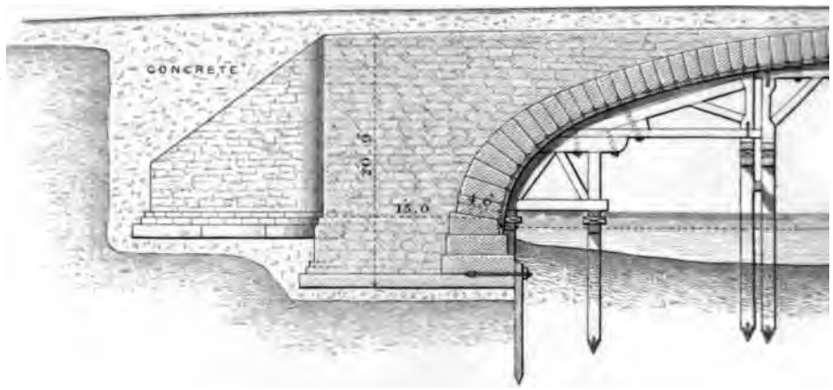


ELEVATION AND PLAN OF PEDESTAL, PARAPET, &c.





CROSS SECTION



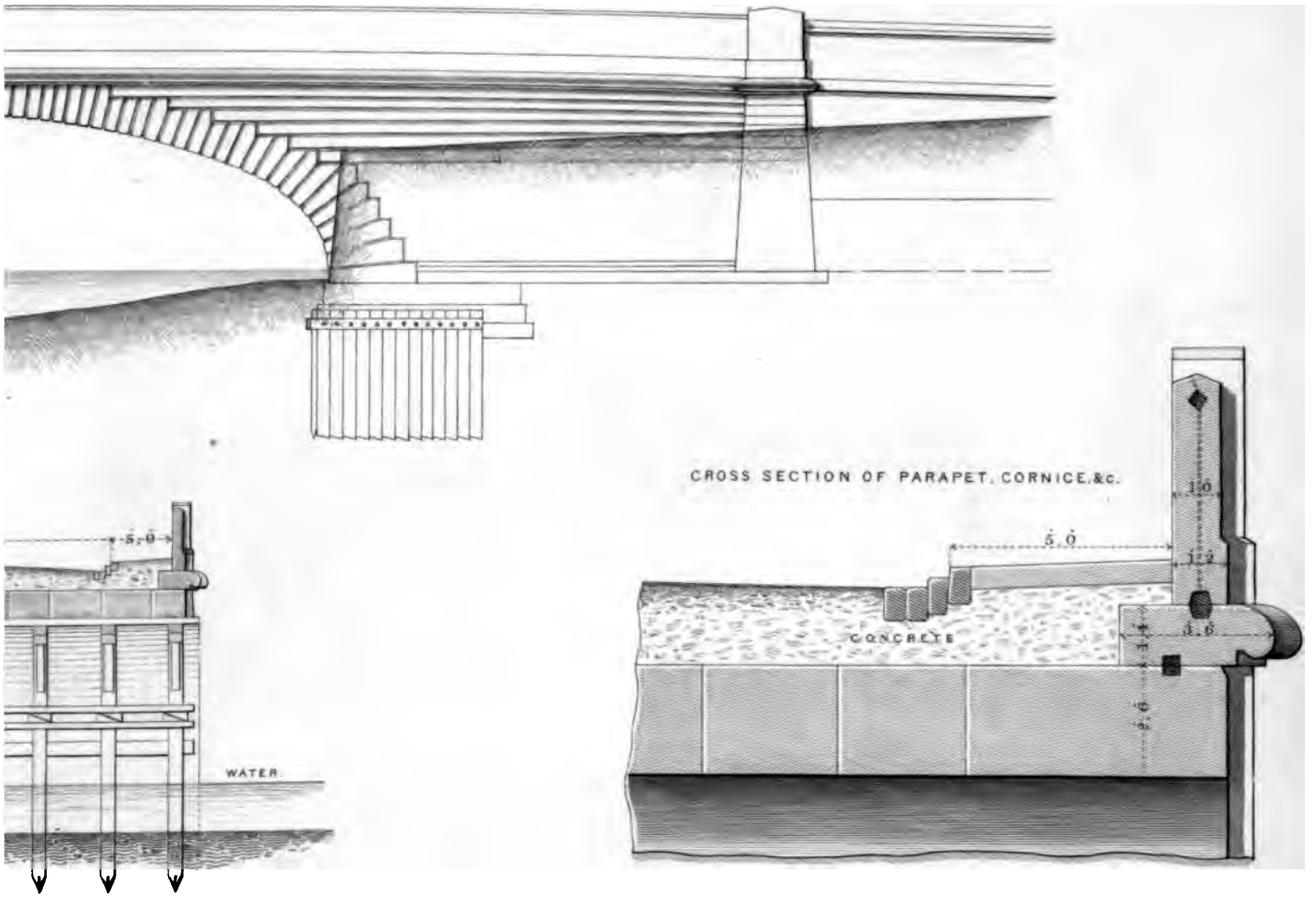
LONGITUDINAL SECT

GENERAL DIMENSIONS OF BRIDGE.

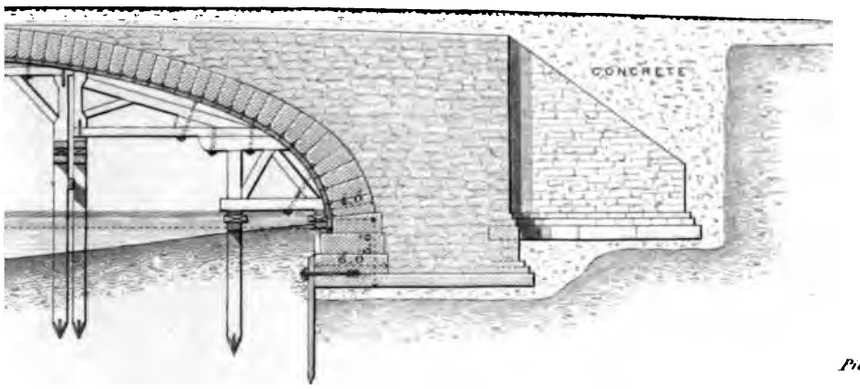
	Ft	In
Span of Arch on Square Line	64	0
Ds on Oblique Line	66	0
Rise of Arch above low Water	13	9½
Total Width of Bridge on Square line	42	7¾
Ds Obliquely	43	10½
Total Length of Ditto	146	0

SCALE OF 10 5 0 20

GE.



VITH CENTERING.



GE AND CENTERING.

SCANTLINGS OF CENTERING.

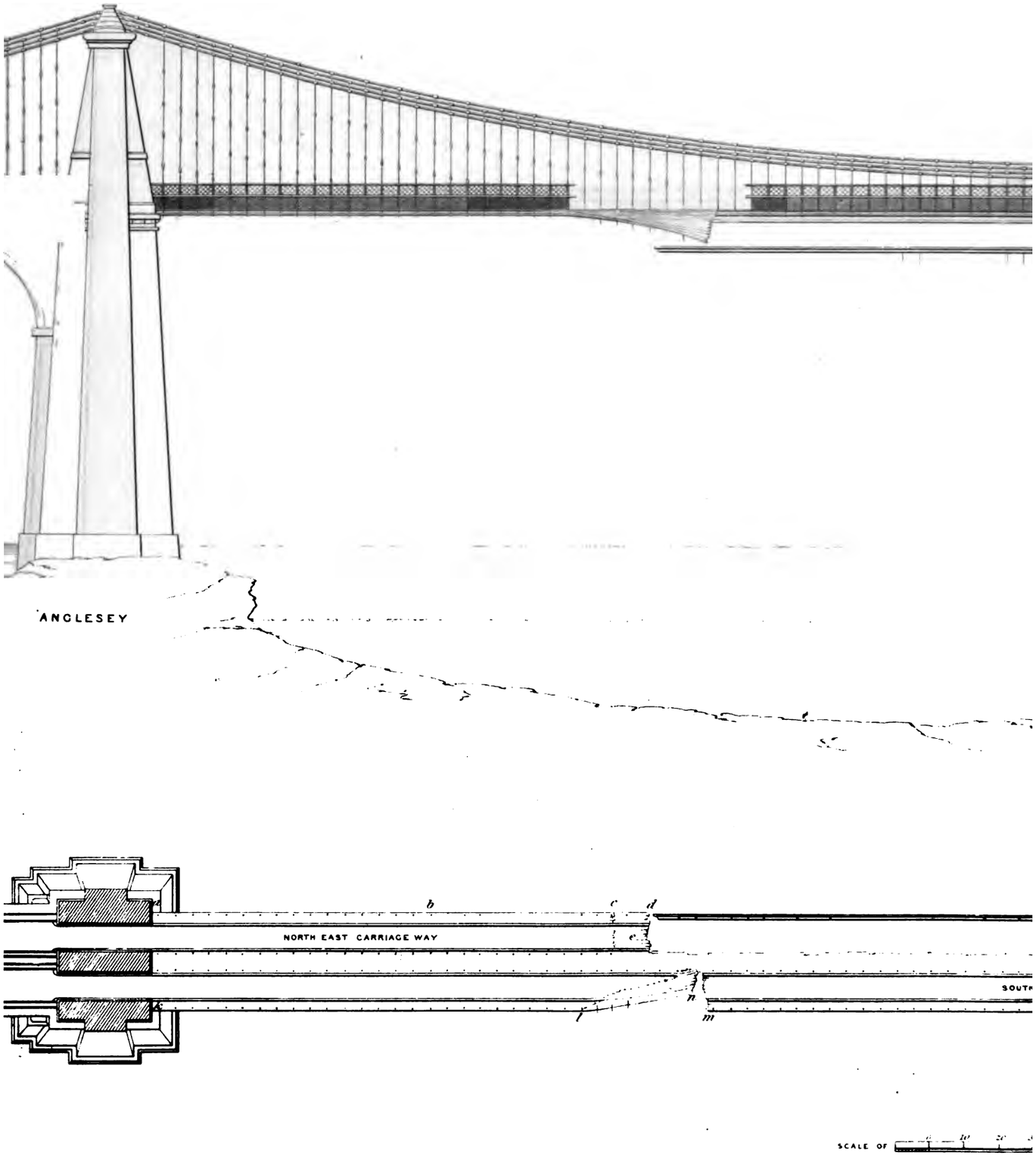
	IN.	IN.
Piles	12	12
Sills	12	6
Principal Framing	12	12
Struts	10	10
Braces	10	5
King Posts	14	12

30 40 50 FEET

30 ENLARGED PORTIONS.

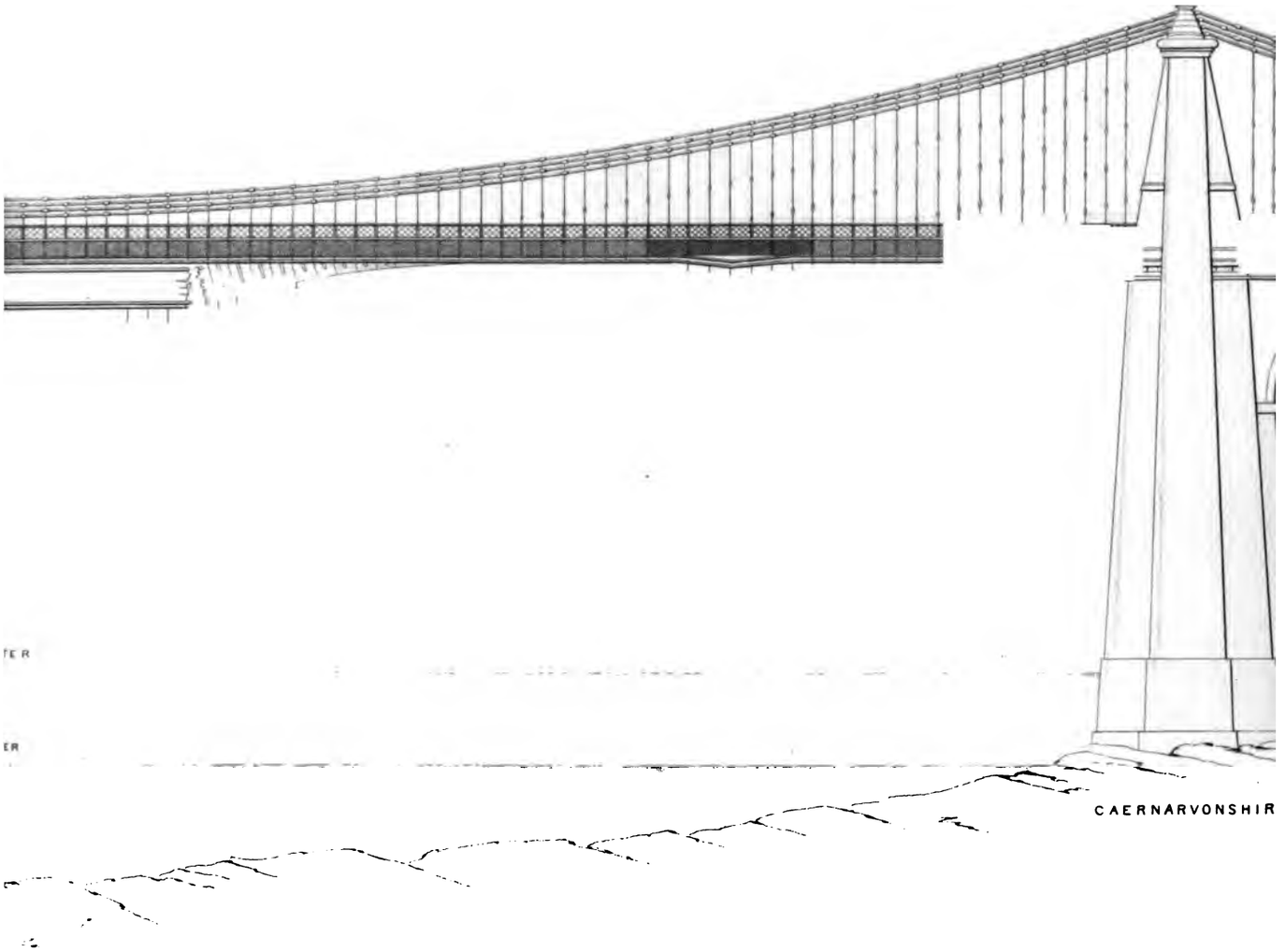
THE M

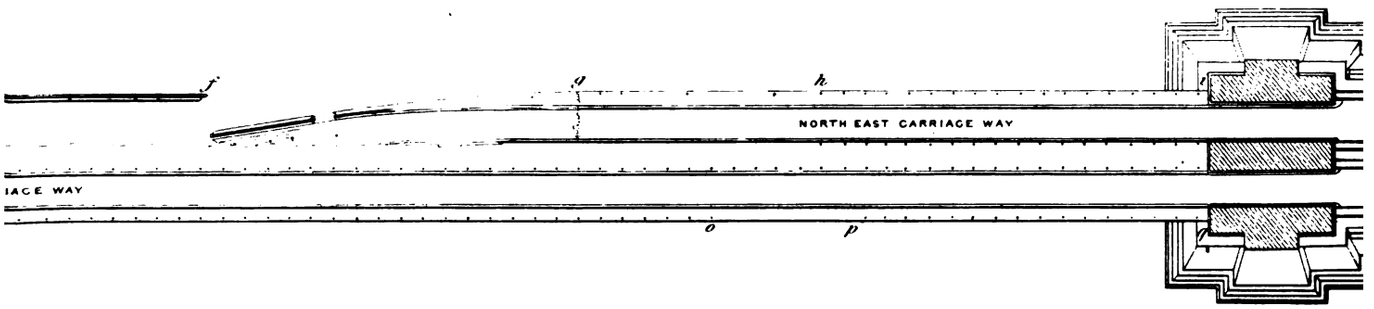
AS IT APPEARED AFT



BRIDGE,

'ORM OF JANUARY 6.7.1839.

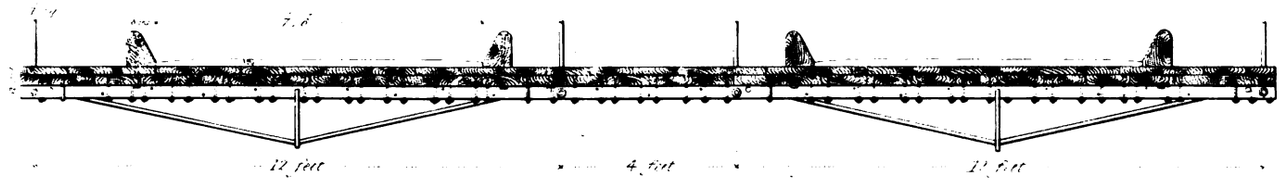




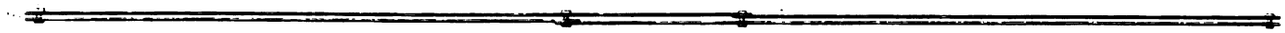
THE MENAI BRIDGE.

SECTIONS THROUGH THE PLATFORM SHEWING ELEVATION OF THE ROADWAY BEARERS.

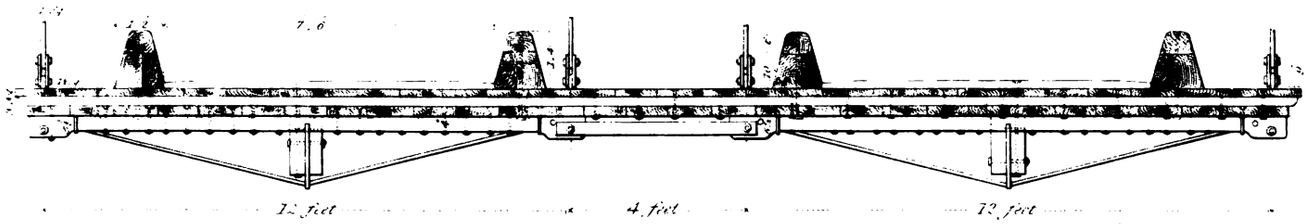
AS ORIGINALLY CONSTRUCTED.



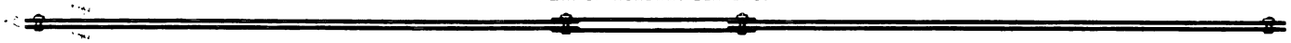
PLAN OF ROADWAY BEARERS.

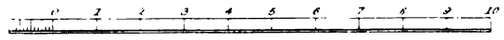


AFTER THE ALTERATIONS.



PLAN OF ROADWAY BEARERS.



SCALE OF 

FEET.

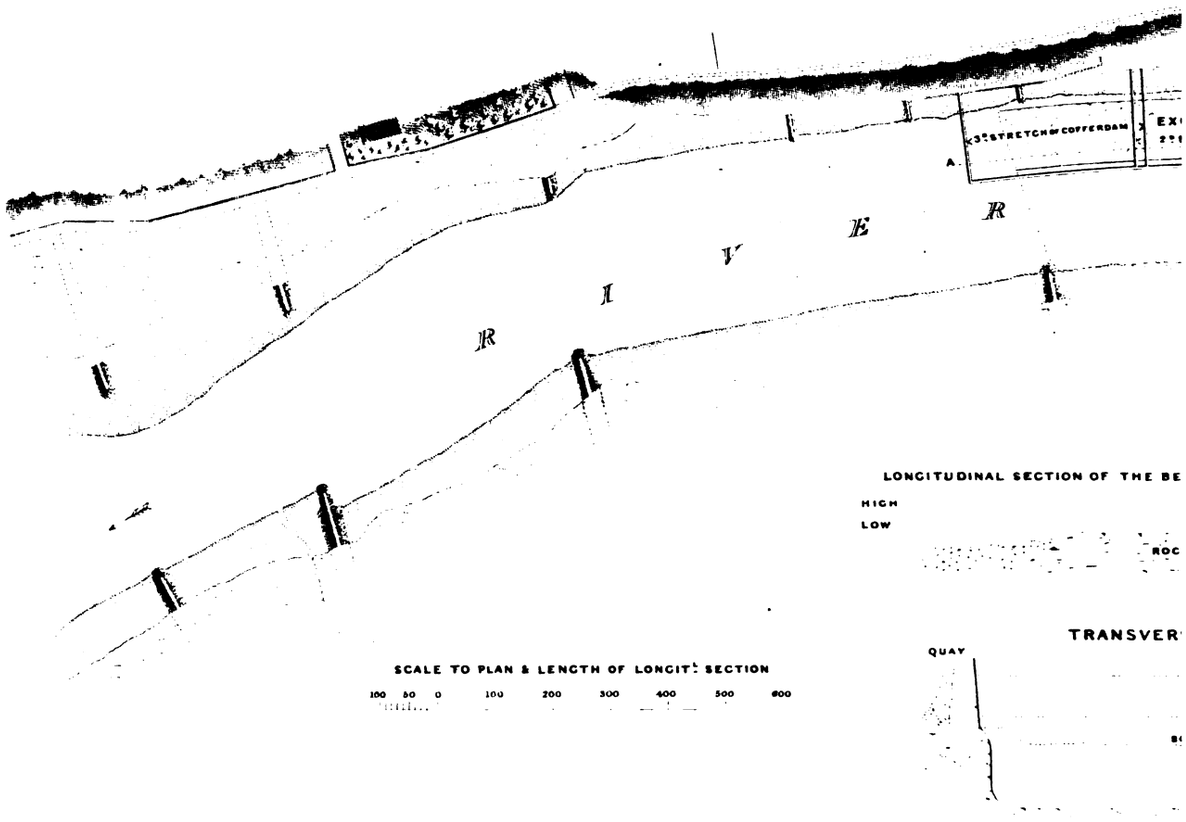
Reduced by G. A. Jermyn, Grad. Ins.t C.E.

T. J. Maude, del.t Institution of Civil Engineers. 1841. *S. Bedin, sculp.t*

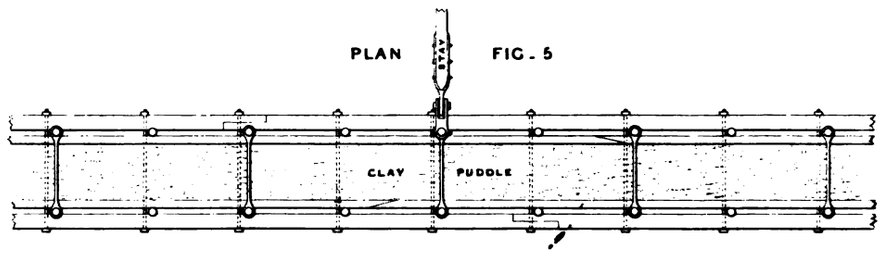
PLATE

COFFER-DAM USED FOR EX

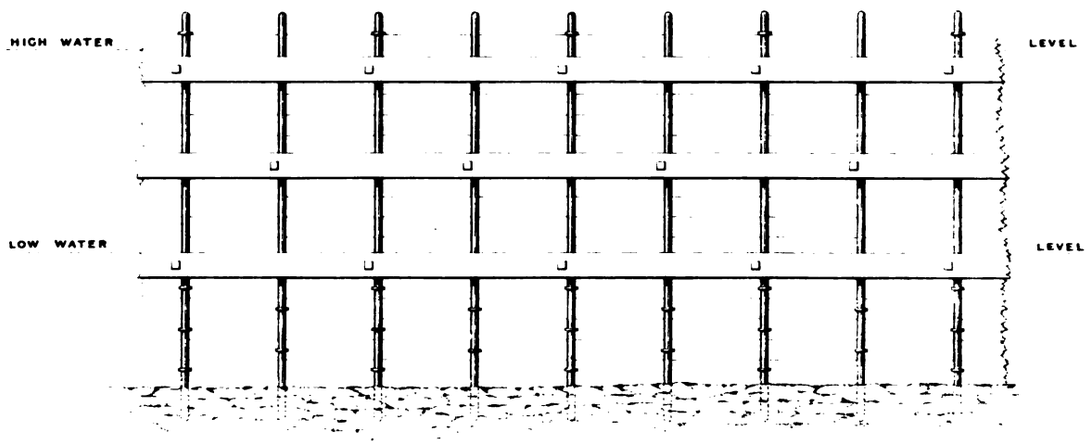
DAVID S



PLAN FIG. 5



ELEVATION FIG. 4



AVIGATION.

ING ROCK IN THE RIVER RIBBLE.

PL.

ENSON. C.E.

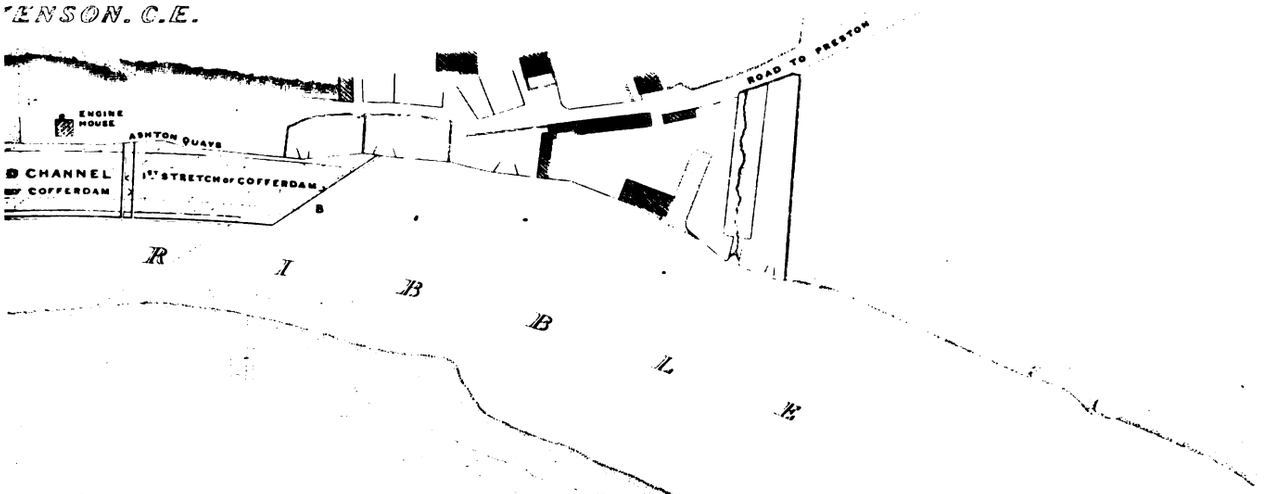
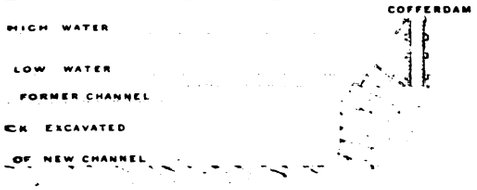


FIG. 2

LE RIVER FROM A. TO B. IN THE LINE OF EXCAVATION

FIG. 3

CTION OF NEW CHANNEL

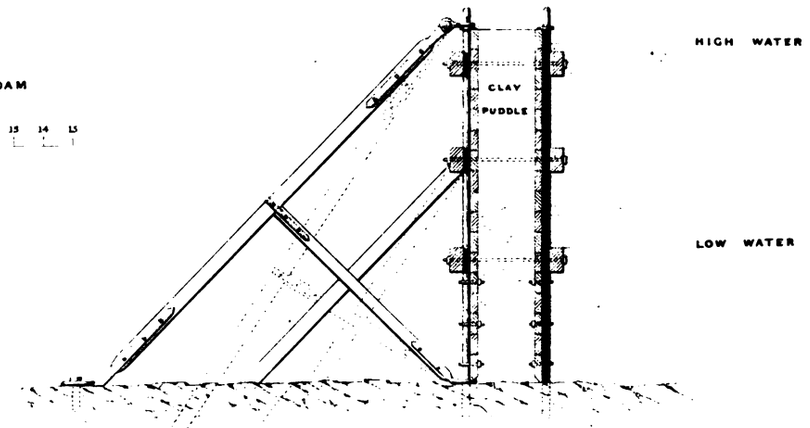


SCALE TO TRANSVERSE SECTION OF CHANNEL

10 5 0 10 20 30 40 50 60

FIG. 6

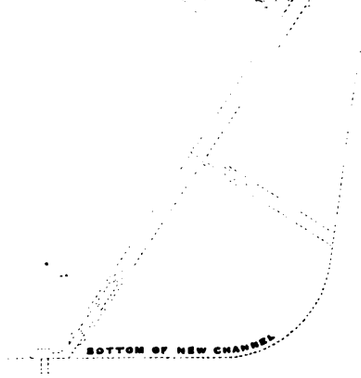
TRANSVERSE SECTION OF COFFERDAM



LE OF FEET TO DETAILS OF COFFERDAM

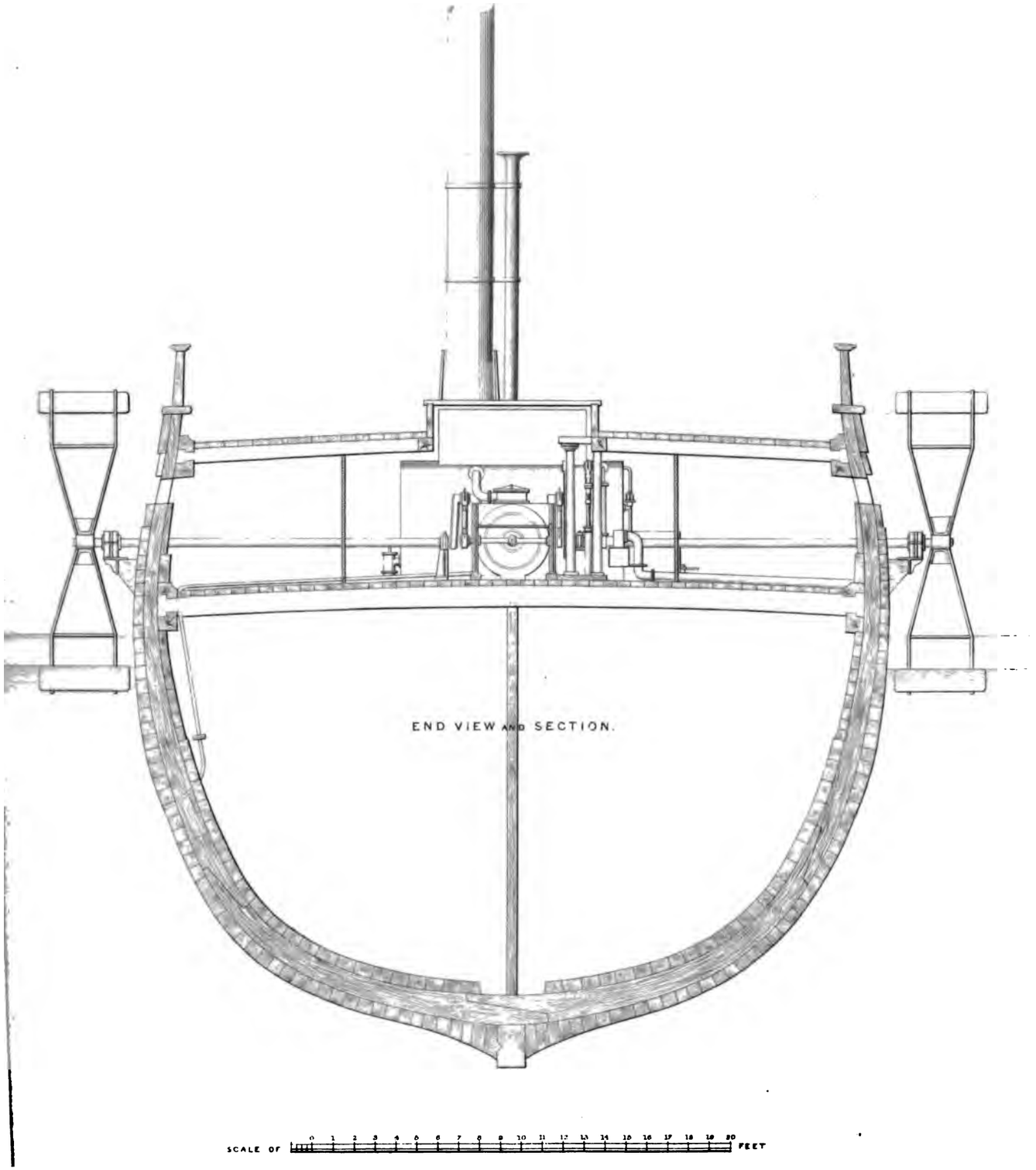
FIG. 4 5 & 6

2 3 4 5 6 7 8 9 10 11 12 13 14 15

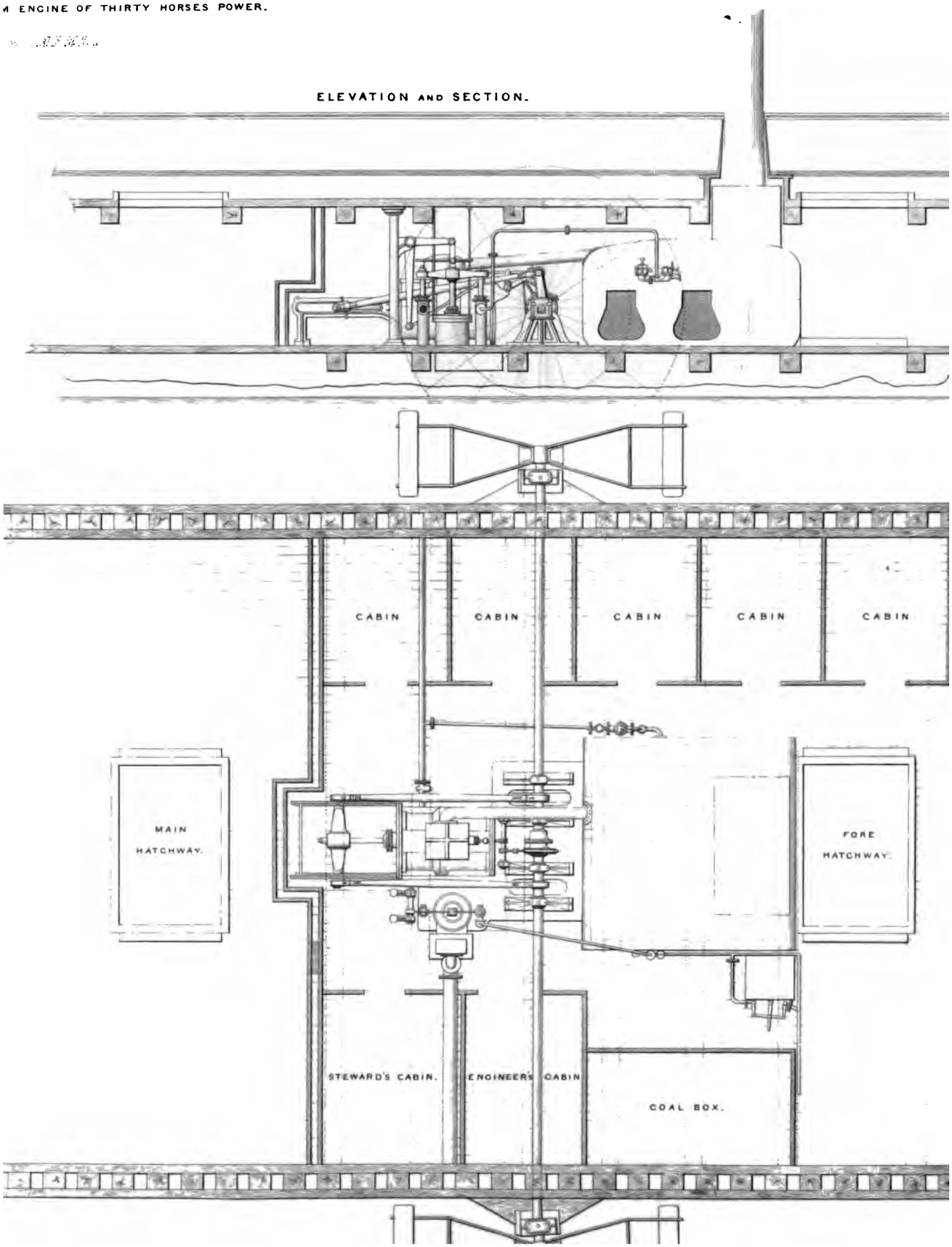


VERNON,

ONE THOUSAND TONS, WITH AN AUXI



INDIA MAN.

A ENGINE OF THIRTY HORSES POWER.

ELEVATION AND SECTION.